# THE GREAT STAR MAP

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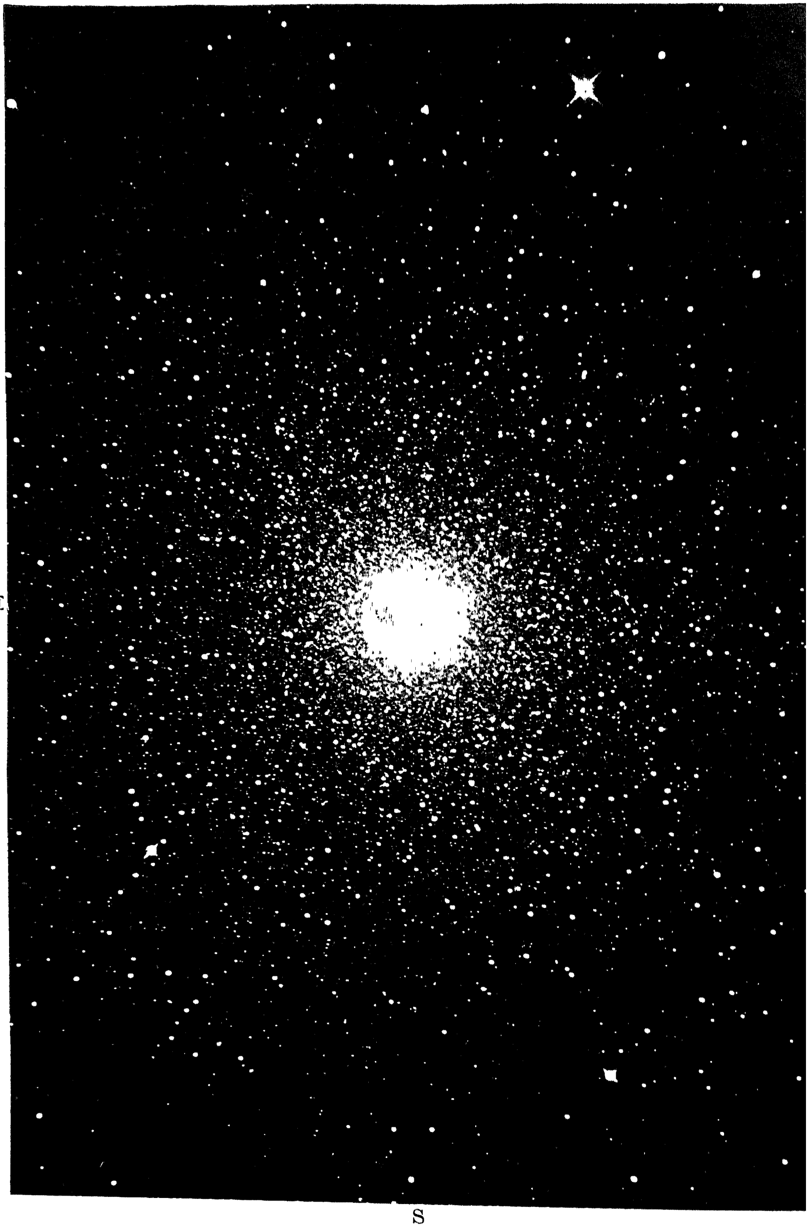
E 

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CLUSTER M.3. CANES VENATICI (N.G.C.5272) BY G. W. RITCHEY. APRIL 9, 1910
MOUNT WILSON OBSERVATORY. 60-in Reflector. Exposure 3½ hours

# THE GREAT STAR MAP

BEING A BRIEF GENERAL ACCOUNT OF THE INTERNATIONAL PROJECT KNOWN AS THE ASTROGRAPHIC CHART

BY H. H. TURNER, D.Sc., D.C.L., F.R.S.

SAVILIAN PROFESSOR OF ASTRONOMY IN THE UNIVERSITY OF OXFORD

*WITH FRONTISPIECE*

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# CONTENTS

# PREFACE

THE main portion of this little book appeared as a series of four articles in *Science Progress*, July 1910 to April 1911: and I am indebted to the Editors and Publisher of that Review for permission to reprint. I must further express my grateful acknowledgments to Mr. A. S. Eddington, Chief Assistant at the Royal Observatory, Greenwich, for his very careful reading of the proof-sheets. An important criticism of his on one point of the argument is considered in Note XII, p. 155.

H. H. T.

UNIVERSITY OBSERVATORY, OXFORD,
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# THE GREAT STAR MAP

## I

## INTRODUCTION

THE simpler name "star map" is here applied to the chart generally known as the "Astrographic Chart," because this latter conveys a suggestion of technicality which is absent from the project. What astronomers in different parts of the world are really about is the making of a large and much more detailed map of the stars than has hitherto been produced. The map is being made by photography; but though the word "astrographic" has been coined for use when photography is applied to the stars, the work does not involve much technicality that is not familiar to the users of an ordinary Kodak. In three details only does the work of the astronomer differ

from that of the amateur photographer: he uses a much longer camera; he drives the camera by clockwork so that it may follow the stars; and he takes pictures at night instead of in the daytime. It may perhaps be added that he uses the light emitted by the stars, instead of photographing objects by the reflected light of the sun. But of these details more presently.

Let us first consider what is the nature of a map of the stars, as this differs somewhat in character from the maps of the earth's surface with which we are familiar. There is no question of finding our way, no question of delimiting property, no question of showing hills and valleys. A map of the stars is of a more monotonous character, being practically limited to showing the exact positions and the brightnesses of individual points of light. Maps of the stars may differ from one another in scale, in accuracy, and in completeness: in scale because we may show two given stars separated on the map either by a foot or by an inch, according to requirements; accuracy will have a tendency to be greater on the larger scale; and

we may indicate either a few bright stars or many faint ones. We are familiar with the fact that there are only a few very bright stars, more of a degree less bright, more still of fainter stars; and the increase continues as the luminosity diminishes, long after they have ceased to be visible to our eyes, no limit being reached even by the longest exposures given with our largest telescopes. Completeness then can only be a relative term. It is at present impossible to think of giving all the stars in the sky; we can only settle to give all those brighter than a certain fixed standard.

The earliest maps of the stars were probably made for astrological purposes; later they were required for the use of sailors. But through all the centuries so little had been done towards making accurate maps that in 1674, when there arose a question of finding the longitude at sea by observations of the moon and stars, it was pointed out by Flamsteed that no sufficiently accurate maps or catalogues of the stars were available. King Charles II., to whom this information was brought, was thoroughly

alarmed at the state of affairs, and said that he must have the omission rectified forthwith. Thus was Greenwich Observatory established. When asked who was to take charge of the Observatory, the King immediately replied that Flamsteed, who had pointed out the need of such an institution, was the man to be put in charge. Modern observation of the position of the stars may be said to have begun at this period. Greenwich took a great step forward half a century later, when Bradley was made the third Astronomer Royal and increased the accuracy of observation very considerably, so that his results have formed the basis of our knowledge of the positions of the stars to the present time. But Bradley and his successors for the most part confined their attention to the brighter stars, not concerning themselves with those much fainter than can be seen with the naked eye. There were two good reasons for this. In the first place, the number of stars required for the use of sailors is not large; indeed, sailors themselves use remarkably few, since only the brightest are suitable for observation by the

small telescopes of their sextants. Indirectly, however, sailors depend upon the keeping of accurate time ("Greenwich time" is in use all the world over for determining longitude): and for keeping accurate time a much larger number of stars, called "clock stars," is required. These have had the first claim upon the attention of astronomers at our great observatories during a couple of centuries. A second reason for confining attention to these brighter stars arises from the limitations of instruments. The observations were generally made by watching the star cross the field of view, in which were certain spider lines for reference. Now these lines cannot be seen unless the field of view is illuminated, and a faint star is then lost in the illumination. In these days of electric light it is comparatively easy to adopt a new instrumental method, whereby the wires themselves (and not the background) are illuminated; they then appear as bright lines but are not sufficiently dazzling to obscure even a faint star, which can thus be observed as well as a bright one. But in former times this method had not been

sufficiently developed and in any case the brighter stars were easier to observe. For these reasons therefore the fainter stars have not attracted attention until comparatively recently. One motive for studying them came with the discovery of the minor planets, which dates from the first day of the nineteenth century. It had been realised that there was a gap in the sequence of planets (as arranged in order of distance from the sun) between Mars and Jupiter. It was clear that there could not be any large planet in this position, for it would have been noticed; but there might be a small one, and search was made for it. The method of search was very laborious, since it was necessary to identify all the stars within a certain region in order that any strange body might be detected. It is now easy to accomplish this by taking a photograph of the region; but at the end of the eighteenth century no such compendious process was available; the positions of individual stars were then either patiently and laboriously measured one by one, or learned by the astronomer so that he could carry a picture

of the region in his memory. In default of an actual material photograph he practically photographed the image on his own retina. It is astonishing to think how much was accomplished by this toilsome process. Not one only, but hundreds of minor planets were discovered in this way, though not without difficulty and delay. Four were found at first in rapid succession, and then came a long blank during nearly half a century, so that it seemed as though the number were complete: but though this view proved quite erroneous, it was only after a search of fifteen years that Hencke, an ex-postmaster of Driessen, was at last rewarded by another discovery. Since that time the number has been extended almost continuously, so that we now know nearly seven hundred of these tiny bodies. From the circumstances attending the discovery and the subsequent observation of them has arisen one need for charting the places of the fainter stars. The easiest way to record the movements of these small bodies is to measure their distances from adjacent faint stars, which can only be effective when we know the places of the

stars themselves. This led astronomers to undertake the great work of charting the zone of the heavens called the Zodiac, in or near which all the planets move. Such an enterprise was started at Berlin early in the nineteenth century; another, initiated by Chacornac many years later, was continued by the brothers Henry of Paris, who ultimately took the great step of employing photography in the work; and this led directly to the inception of the scheme we are now considering.

The introduction of the photographic method was at first fitful and tentative. Apparently the earliest attempts were made in America by the Bonds and by Rutherfurd. It is curions now to read of the difficulties in obtaining impressions of any but the brightest stars in the old days of wet plates. The wet plate of course was not nearly so sensitive as the dry plate; also it could only be exposed for a limited time before it dried up, and during such limited exposures only the brightest stars left an image upon it. Even the wildest hopes of these early pioneers in forecasting the future fell

far short of what is now easily attainable: witness the following extract[1] from a letter of George Bond to the Hon. William Mitchell, Nantucket, dated from Cambridge (Mass.) July 6, 1857:

"As far as I am informed, the attempt to photograph the fixed stars by their own light has been made nowhere else up to the present date. The rumour of a daguerreotype of a nebula made in Italy some years since, was unfounded. . . .

"About seven years since (July 17, 1850) Mr. Whipple obtained daguerreotype impressions from the image of *α Lyræ* formed in the focus of the great equatorial and subsequently from *Castor*, thus establishing a simple but not uninteresting fact—the possibility of such an achievement. On these occasions a long exposure of one or two minutes was required before the plate was acted upon by the light. . . .

"Messrs. Whipple and Black recommenced their trials on other images (taken

[1] *Memorials of William Cranch Bond, Director of the Harvard College Observatory* 1840–59, *and of his son George Phillips Bond, Director of the Harvard College Observatory* 1859–65, by Edward S. Holden (Lemcke & Buechner, New York, 1897), p. 155.

by the collodion process) in March of the present year and they are still in progress. . . . Could another step in advance be taken equal to that gained since 1850, the consequences could not fail of being of incalculable importance in astronomy. The same object *a Lyræ*, which in 1850 required $100^s$ to impart its image to the plate, and even then imperfectly, is now photographed *instantaneously* with a symmetrical disc fit for exact micrometer measurement. We then were confined to a dozen or two of the brightest stars whereas now we take all that are visible to the naked eye. Even from week to week we can distinguish decided progress. . . . At present the chief object of attention must be to improve the sensitiveness of the plates, to which I am assured by high authorities in chemistry there is scarcely any limit to be put in point of theory. Suppose we are able finally to obtain pictures of seventh magnitude stars. It is reasonable to suppose that on some lofty mountain and in a purer atmosphere we might, with the same telescope, include the eighth magnitude. To increase the size of the telescope threefold in aperture is a practicable thing if money can be found. This would increase the brightness of the stellar images, say

eightfold, and we should be able then to photograph all the stars to the tenth and eleventh magnitude inclusive. There is nothing then so extravagant in predicting a future application of photography to stellar astronomy on a most magnificent scale.

. . . . .

"P.S.—I find I have forgotten to allude to two important features in stellar photography—one is that the intensity and size of the images taken in connection with the length of time during which the plate has been exposed measures the relative magnitudes of the stars. The other point is that the measurements of distances and angles of position of the double stars from the plates, we have ascertained by many trials on our earliest impressions, to be as exact as the best micrometric work."

The letter is a remarkable one for the date. The three forecasts of improvement —increased sensitiveness in plates, larger instruments, and better climate—have all been realised within fifty years. There are two mountain observatories in California; there is a 40-inch lens, nearly three times the size of the 15-inch Harvard equatorial, at the

Yerkes Observatory, and two 5-foot mirrors represent an even greater advance; there has been also an enormous increase in sensitiveness of plates. It was in this last particular that Bond failed to allow sufficient play to his imagination, as instead of an increase represented by one stellar magnitude we have more than ten times that estimate. But Bond's discernment was otherwise so great that this slight failure may be pardoned. His postscript shows that he realised even thus early the accuracy of the photographic method, and in this his judgment agreed with that of L. M. Rutherfurd, who set to work to measure his photographs systematically and soon found that they recorded the positions of the stars more accurately than the best apparatus would measure them. In using a micrometer screw he found, though he had provided himself with the best one available, that its errors were sufficiently large to prevent his doing justice to the photographs. He therefore turned aside from his original project to the construction of a better screw, and ultimately made a screw so accurate that his attention

was again distracted towards the completest possible test of its accuracy. This he found in the ruling of very fine lines close together on metal—several thousands within an inch—the result being what is called a grating, which can be used like a prism to spread out light into a spectrum. This work was so engrossing that Rutherfurd never seriously returned to his original purpose of measuring star photographs; but many of his have been measured since, and have shown clearly how correct was his judgment of the accuracy of the photographic method. In spite of this accuracy, however, the inconvenience of the wet plate long delayed serious use of the method for the determination of star places. Photographs of the sun (requiring only a momentary exposure) were taken showing the spots, and measures of spot positions were made on these and found satisfactory. But a sun spot is an irregular object having no very definite position and does not afford a very severe test of accuracy; consequently this work failed to draw the attention of astronomers to the full resources at their command.

The complete change in attitude came in a rather sensational manner on the appearance of the great comet of 1882. This comet, which was quite a respectable object in the Northern Hemisphere, was much more magnificent in the Southern. The dry plate had by this time made photography easy, and many members of the public who had become possessors of cameras essayed to photograph the comet only to be disappointed by finding that the rotation of the earth, carrying them and their cameras with it, was sufficient to spoil their pictures. Thereupon Sir David Gill, then H.M. Astronomer at the Cape, invited one of them to come to the Observatory and to strap his camera to the equatorial telescope (which was fitted with clockwork to counteract the earth's motion); immediately some beautiful pictures of the comet were obtained, and not only of the comet but of the surrounding stars. The number of stars shown on the photographs was indeed striking, and attracted widespread attention. The late Dr. Common of Ealing, who had been constructing telescopes for himself, without

however any definite intention of using them photographically, immediately turned them to this new purpose and obtained some beautiful pictures of nebulæ. The brothers Henry in Paris saw the possibility of substituting the new process for the immensely laborious method by which they had been making their ecliptic charts; but in their case the change could not be made so easily, as their telescope had been made for visual work and could not immediately be used photographically. The difficulty arises from the existence of numerous colours in white light, the colours with which we are familiar in the rainbow. When looking through a telescope with the eye we use chiefly rays nearly yellow in colour, whilst the photographic plate is sensitive to blue and violet. Now a lens cannot be constructed to focus all these rays at the same time, and consequently for photography a new lens must be made which will focus the blue and violet light instead of the yellow. There are ways of avoiding this difficulty which may be briefly mentioned. In the first place if we use a mirror which brings the rays to focus

by reflection, instead of a lens which combines them by refraction, no colour difficulty arises. (It was for this reason that Dr. Common was able to use at once for photography the reflecting telescope which he had originally built for eye observation.) Secondly, modern improvements in the construction of photographic plates have made them sensitive to yellow light under certain conditions, so that visual telescopes can be used to take photographs if a yellow screen cuts out the unfocussed blue rays, leaving only those for which the telescope has been properly focussed. When a suitable plate is then put behind the screen, pictures of the moon and stars can be and have been obtained quite as good as those obtained with a telescope specially made for photography. But in 1882 this had not been realised, and the Brothers Henry saw no way of using the new and promising photographic method but to make a new lens specially adapted for it. This they set about with great skill and determination. After a few trials on small lenses they at last succeeded in producing a photographic lens

of 13 inches aperture, a veritable triumph of optical workmanship at that time. They were of course amateurs at the work. Admiral Mouchez, the Director of the Paris Observatory, gave them every encouragement and put at their disposal such resources as he had available; but their workshop was after all a mere shed. I have often heard Dr. Common speak with amusement of his visit to the workshop which had turned out to the admiration of the world the first successful photographic refractor—the modest building and the humble appliances were so surprising. We are reminded of the simple apparatus with which great experimenters like Faraday have often achieved the most remarkable results.

It was the work of the lens thus produced by the Henrys that led directly to the inception of the project we are considering. The specimen maps of small regions of the sky which they soon obtained suggested the possibility of producing such maps for the whole sky. The work contemplated was no child's play. At least 10,000 maps would be required to cover the whole sky; and a

labour of this magnitude was beyond the resources of a single observatory. Correspondence between Sir David Gill—under whose direction the comet photographs had been taken—and Admiral Mouchez, who had encouraged the work of the Henrys, led ultimately to the assembling of a great international Conference at Paris in 1887. It was a remarkable meeting, the first of its kind in the history of astronomy; and it has shown the way for subsequent gatherings which have already made their mark upon that history. Conferences of a similar kind have since been held in 1889, 1891, 1896, 1900; and after a long interval in 1909. On all these occasions the French have acted as hosts and have discharged these duties with a cordiality and hospitality that has never failed to impress their colleagues from the most distant parts of the world. It would be difficult indeed to imagine a more pleasing centre for our meetings than Paris, or a nation more admirably adapted to play the part of hosts than the French; and they have been rewarded by an increasing success in the gatherings. At the last meeting it

became clear that the assembly had developed from a mere collection of those interested in a particular project into an organisation of the world's resources for the promotion of the astronomy of position. The physical side of astronomy has recently been organised on somewhat similar lines (profiting no doubt by the example provided), and the existence of these two great organisations will have a notable effect in economising our labours in the future. In 1887 such an important outcome was scarcely anticipated: attention was then concentrated on the immediate task before the assembly, which was a difficult one in every way. Astronomers from distant quarters of the globe,[1] speaking different languages, none of them with much experience of photography or of its possibilities, but most of them with opinions more or less formed, met together to try and secure unanimity, not only in generalities but equally in small details. We need not be surprised at some of the results. The discussions were, to say the least of it, animated. There are no uni-

[1] See Note I.

versal rules for conducting public business, and astronomers of one country were not familiar with rules in use elsewhere. It interested Englishmen, for instance, who are accustomed to have resolutions moved by any one rather than the chairman, to learn that this was by no means a universal rule. On the contrary, the chairman of the first conference considered it part of his duties to move all the resolutions. After listening to a discussion, he took it to be his function to summarise the sense of the meeting in a resolution which he put from the chair and in favour of which he held up his own hand. Unfortunately for his success his was sometimes the only hand held up, and the discussion was necessarily resumed. Another feature of such discussions on the Continent is a little strange to our insular prejudices but might perhaps be adopted by us with advantage. Occasions sometimes arise when the collision of contrary opinions produces considerable heat, and there is an obvious desire on the part of two gentlemen (or even more) to speak at the same time. On such occasions the chairman rings a bell and

declares the sitting intermitted for a few minutes. What has been public discussion can now be developed as private conversation. Expositors of opposite views who have been addressing one another excitedly across the width of the room may now rush together and arrive at a better understanding at close quarters. The effect of such an opportunity soon becomes evident when after a few minutes' interval the chairman again rings his bell—a calm has succeeded to the storm and not infrequently it is possible to crystallise out a resolution.

Let us glance at one or two of the matters which had to be decided in 1887.[1] The first and most important was the choice of an instrument or instruments—for it was a preliminary question whether the same pattern should be used by all those co-operating in the work. This preliminary question, however, was soon settled in the affirmative. All were to use similar instruments; and now what were they to be? Should they be reflecting telescopes, as used by Dr. Common, refracting telescopes as made by the

[1] See Note II.

Brothers Henry, or refracting telescopes of a different pattern, and more closely similar to camera lenses as advocated by Professor Pickering of Harvard ?

The advantages of the reflector were that it was cheap and that it existed. It is cheap because there is only one surface to be polished. Reflectors used to be made of speculum metal polished to a concave form ; such were, for example, the great telescopes of Sir William Herschel and of Lord Rosse: nowadays instead of metal we use glass silvered on the face (not on the back as in a domestic looking-glass) : but in either case there is only one surface to be prepared optically. Now with lenses there are two, four, or even more surfaces, all of which must be optically true. Moreover the glass must be entirely free from blemishes ; if there is a fault in the substance of the glass which forms a mirror it is behind the reflecting surface and may not spoil the image ; but a fault in the interior of a lens cannot fail to produce its effect. Hence a lens is always much more costly than a mirror of the same size, and the greatest telescopes

in the world have always been reflecting telescopes. Lord Rosse's 6-foot mirror has not yet been surpassed in size, although Dr. Common and Dr. Ritchey have both succeeded in making mirrors of 5 feet, and a mirror of no less than 8½ feet diameter is proposed; but the largest *lens* in the world is the Yerkes of 40 inches. Hence it could not fail to impress the conference of 1887 that the more economical instrument would be a reflector; moreover several such reflectors were already in existence and could, so it was hoped, be utilised without further expense. Thus at Oxford there was a reflecting telescope, which Dr. De la Rue had presented to the University Observatory, and with which Professor Pritchard hoped to take a share in the project: if it were decided to use a different pattern of instrument his hopes would be disappointed unless he could obtain the money necessary to purchase one of the adopted pattern.

As regards the two forms of refracting telescope, the refractor and the doublet, that advocated by Professor Pickering was the more expensive and the less known. In the

light of our modern knowledge of its advantages (especially for the purpose of covering a larger area of the sky at once) it is very strange to find so little in support of it in the accounts of the discussion. It seems to have been put aside almost at once, in spite of the letter urging its adoption from Professor Pickering. The chief reason for this was undoubtedly lack of information as to the accuracy with which plates taken by such an instrument would give the places of the stars. Specimen photographs taken by the Brothers Henry with the other form of refractor had been measured and shown to be very satisfactory, but there was no corresponding information about the "doublet" as this third form of instrument is now usually called. Hence the doublet was put aside from the start and the choice was made between the reflector and the simple refractor.

The decision fell upon the latter. The choice has proved to be a wise one and it is satisfactory to remember that it was made without any acrimonious discussion. This was largely due to Dr. Common himself,

who might perhaps have been expected to lay stress on the particular advantages of his own special instrument. His experience however had impressed him rather with its defects, especially with its uncertainty. This uncertainty is not due to the instrument itself so much as to our fitful climate: the reflector is so seriously influenced at times by air currents and changes of temperature as to be an instrument of moods and Dr. Common has accordingly compared it, somewhat ungallantly, to the female sex. He himself took the initiative in recognising that the Conference should adopt for a work of such magnitude the more trustworthy refractor as made by the Brothers Henry; a straightforward course which had its due effect on the formulation of a decision. There are now therefore a score [1] of such instruments scattered about the world, varying a little in non-essentials but all closely resembling one another in the size of the lens (which is $13\frac{1}{2}$ inches in diameter) and in the focal length of the telescope (which is about $11\frac{1}{4}$ feet). The focal length is actually defined

[1] See Note III.

to be that which represents one minute of arc by a millimetre on the photographic plate. This simple relation between the minute and the millimetre was suggested by the brothers Henry, and it has been found so useful that it has been retained in other cases. For instance Dr. Common made his 30 inch mirrors of this same focal length—the mirrors at Greenwich and at Helwan (in Egypt), which have recently taken such beautiful pictures of comets, and shown their power of detecting very faint satellites.

Another very important decision taken by the Conference of 1887 had a rather curious history. It arose from the ignorance, at that time, of the behaviour of a photographic film and the fear lest it should shrink in drying or otherwise become distorted. Experience of photography generally—as for instance the taking of portraits or landscapes—was sufficient to show that such distortion was at any rate not large; but in astronomy we are concerned with very minute quantities, and it was not known whether minute disturbances might not affect the relative positions of the images

on the plate. Accordingly it was proposed to imprint upon each plate a series of accurately ruled cross-lines called a *reseau*. They were to be photographed on the plate before development by exposing it to an artificial light behind a silver matrix (a flat plate coated with silver ruled with such lines); on development the lines appear together with the star images, and if the film has shrunk during any of the processes of development, fixing, washing, etc., these lines will have shrunk sympathetically and will be no longer straight or at exactly equal distances as they were in the matrix. We have now learned that such shrinkage is so very small as to be negligible, at any rate for the purposes of our star map; indeed, even in the most minute investigations it is easier to neglect the shrinkage as accidental in character than to investigate it. Accidental errors can be obviated by taking another plate (or a number of other plates) and so far as our present experience goes the whole series of plates is very unlikely to be affected by any common or systematic error. Hence the function assigned to the

*reseau* was due to a misapprehension and it has never been used for the purpose originally proposed. Fortunately it has been of immense value in another way. The lines have served as reference marks in determining the places of the stars with facility. To measure the distance between one image and another we might have used a long screw carrying a microscope. Screws can now be made very accurately if necessary; Rutherfurd's work laid the foundations of such accuracy. But they are costly; their use over a large range takes time in turning the screw through many revolutions; and continual use is apt to wear away the screw and render it no longer accurate. Hence it is preferable to use another method in which the distance to be measured is compared with an accurately divided scale, a screw being used over a short distance only to connect the stars with divisions of the scale: and the *reseau* has practically supplied an accurate scale in both directions for the rapid measurement of star positions on the plate.

We may pause here to remark that the

term "map" when applied to the present project must be used in a rather comprehensive sense. The scheme includes not only the pictorial representations on the plates or on any prints made from them, but also the measurement of these plates and the publication of the measures of the individual stars. We can if preferred use a descriptive name for these measures. The printed books containing them are often called the Astrographic Catalogue as opposed to the prints, which are the Astrographic Chart proper; but the whole project is really one and the same, although the usual process adopted in making a terrestrial map is here inverted. Surveyors of the face of the earth make careful measurements first and then plot them on a map; that too was the method of astronomers before the days of photography. Now, however, we first take photographs and then measure them; but the project would be incomplete without both measures and charts. An illustration may be given of the risk involved in using one of these methods alone from the practice of Egyptian surveyors. They have been

accustomed by centuries of tradition to enter their measurements of land in books without proceeding to make a map. It is only within the last few years that the Eygptian survey under Captain Lyons made maps for the first time of the landed property in Egypt; and when these beautiful maps were exhibited in Cairo thousands of land-owners saw their property thus represented for the first time. When the maps came to be made the disadvantages of the old plan soon became apparent; some pieces of land had been recorded twice over while others had been omitted altogether. We can easily see how these mistakes crept into the numerical records, though it is not quite so easy to understand how one class of them at least remained undetected. That those who escaped from taxes altogether made no complaint is intelligible, but what of those who paid twice over? We get a glimpse of the submissiveness of the East to their rulers.

# II

## STAR COUNTING

THE map is to be a record of the positions of all stars brighter than a certain standard, and will indicate the approximate brightness of each star. Before following the history of the project further, it is desirable to consider what are the problems which may be solved by the accumulation of such information and cannot be solved without it.

What can we learn, it may well be asked, of the great universe of stars from observations made under the severe restrictions which limit astronomers? We are permanently bound to a small satellite attendant upon one of the stars; other stars are at distances so vast that their movements are only discernible with difficulty: can we learn anything at all about their arrangement in space?

At first sight the inquiry might seem well-

nigh hopeless, but with a little persistence we find that the chances of learning some essential facts are not to be despised: some, it is true, can be learnt only after centuries of labour, but there are one or two which have been established without very much trouble. For instance, it does not take long to satisfy ourselves that the stars are not scattered simply at random through space: it may take a long time to find out what their particular arrangement is, but we feel confident that there is some arrangement for reasons which may be indicated as follows.

The stars have been divided into classes according to their brightness, such that each class (or "magnitude") is fainter than the one above it in a constant ratio. There is of course no sharp distinction obvious in the sky between one class and the next: the brightnesses vary by imperceptible steps, the abruptness of class division being entirely artificial. But it will make the argument simpler, and obscure nothing, if for the moment we suppose these class divisions made quite abruptly: let us imagine all the

stars in each class to be exactly of the average brightness of the class, instead of grading off by small stages into the classes above and below. Now there is overwhelming evidence that these differences in brightness are partly actual differences in the stars themselves and partly the effect of distance. It is certain that the stars are not all at the same distance from us; it is just as certain that, if they were, they would not appear of the same brightness. Taking any particular star of magnitude 2 say and distance 10, we could make it appear of the 3rd magnitude by removing it to distance 16, of the 4th magnitude by removing it to distance 25, of the 5th to distance 40 and so on. Let us suppose spherical surfaces described about the earth with radii proportional to

10 16 25 40 63 100 160 250 etc.

[This series is determined by the convention about star magnitudes and we need not stop to explain it: but it will be noticed that after five terms it is repeated on ten times the scale; there is no difficulty in continuing

it indefinitely both ways by means of this principle.] And now let us suppose all the stars in the neighbourhood of these successive surfaces to be actually collected upon them, which will save us the inconvenience of intermediate grades. Then if the stars had happened to be all of the same intrinsic brightness, those on the first surface (with radius 10) would appear to us of the 2nd magnitude; on the second surface (16) of the 3rd; on the third surface (25) of the 4th and so on. The difference in magnitude would be purely apparent and simply an effect of distance. This, as already remarked, is far from being the case; but before dismissing the possibility we will consider an important consequence of it.

The number of stars on the successive surfaces will increase rapidly outwards. The surfaces themselves increase in area and the distances between them also increase: so that if the stars are scattered through space impartially, the number due to each surface will increase from both causes. A little calculation shows that the resulting increase is as the cube of the radius, so that if there

were 1,000 (or $10^3$) stars on the first shell of radius 10, we should find 4,096 (or $16^3$) on the next shell of radius 16, which is about 4 times as many : on the next shell of radius 25 we should find 15,625 (or $25^3$), which is again about 4 times the number. Had we taken more accurate figures for the successive distances, instead of only approximate values, we should have found a constant ratio, slightly less than 4, for the numbers on successive surfaces : that is to say, that on this erroneous hypothesis of stellar brightness being merely an effect of distance, we should expect to find 4 times as many stars of the 3rd magnitude as of the 2nd : 4 times as many of the 4th as of the 3rd : 4 times as many again of the 5th : and so on continually. Now this expectation is not fulfilled : the ratio is nearer 3 than 4, as the following figures (taken from Newcomb's *The Stars : a Study of the Universe*, p. 54) show :

| Magnitude. | Number of Stars. | Ratio to Preceding. |
|---|---|---|
| 2 | 52 | — |
| 3 | 157 | 3·01 |
| 4 | 506 | 3·22 |
| 5 | 1740 | 3·46 |
| 6 | 5171 | 2·97 |

[We begin with the second magnitude because brighter stars are so few that the numbers have an accidental character.]

What reason can be assigned for this discrepancy between expectation and observation? The one first to be suspected is that the considerable assumption just made, that the stars are all of the same intrinsic brightness, is not correct and is answerable for the discrepancy. But on examination we very soon find that error in this assumption can only increase the discrepancy and is without effect in diminishing it. Suppose for simplicity there were two kinds of stars, one much brighter intrinsically than the other. It will remind us that the difference is in the stars themselves, not an effect of distance, if we use two special words such as "brilliant" and "glowing" to distinguish them. Then in the first shell there will be say 50 brilliant and 50 glowing stars (the numbers are only illustrative). Of these the brilliant stars will appear of the second mag nitude say, the glowing stars of the third. We shall thus recognise 50 stars only of the second apparent magnitude, for the more

distant brilliant stars will be fainter than this and the glowing ones fainter still. Coming to the second shell, we should expect to find $4 \times 50$ or 200 brilliant stars which would now appear as of the 3rd magnitude; and $4 \times 50$ glowing stars appearing of the 4th. Thus altogether we should recognise as of the 3rd magnitude the 200 brilliant stars of the second shell and the 50 glowing stars of the first, making 250 or 5 times the 50 of the 2nd magnitude. Splitting up the stars into two classes has thus enhanced the expected ratio 4 in this instance and made it 5. If we go to the next magnitude we shall find that the ratio returns to 4 and remains at 4 ever afterwards: it is therefore only altered for one step, but this alteration is an increase; there is no diminution available for explaining the observed drop towards 3.

We have taken a very simple case[1]: but its characteristics are maintained in the most complex cases we can devise. They may be stated thus: just as the ratio 4 was disturbed for the first two magnitudes by dividing the stars into two classes, so if it be

[1] See Note XII.

assumed that there are $n$ classes of diminishing intrinsic brilliance (according to steps of a magnitude each), the ratio will be disturbed for the first $n$ magnitudes, after which it will return to 4. In whatever way it be disturbed, it is increased and not diminished.

This avenue of escape is therefore closed and another must be found. Perhaps the figures used are wrong? It is not likely that the counts are wrong, for they have been gone over many times; but is it certain that the drop of a magnitude in brightness is identified correctly? Accurate measures of brightness are not easy to make, as we find in everyday life in connection with candle-power tests: they are harder still for faint lights such as the stars and the difficulties increase as we pass to fainter and fainter stars. We shall presently have to consider these difficulties in connection with the project of the Great Star Map itself. But for the moment it need only be pointed out that it seems unlikely that the discrepancy under investigation is attributable to such a cause. It is easy to calculate what must be the error in estimation of a whole

magnitude if such were the case: to make the ratio $3\frac{1}{4}$ instead of 4 we should have to be 20 per cent. wrong in the measure of magnitude, so that we should be estimating erroneously as a difference of 6 magnitudes what was really only 5. No human measures are perfect but, for reasons which it would take too long to give here, it is practically certain that our estimate is not so wrong as this.

We must go back to an earlier assumption that the stars are scattered impartially through space; this cannot be the case, at any rate in the neighbourhood of our sun. We have so far been considering only the brighter stars (roughly speaking those visible to the naked eye) and these must be nearer to us (other things being equal) than the fainter. It is after all not unnatural that in the neighbourhood of our sun the stars should not be scattered at random: for we see in the sky many "clusters" of stars and it is not unreasonable to suppose that our sun may belong to such a cluster or cloud of stars. The result would be an excess of stars near us and therefore bright:

and to see that this will explain the observed facts we have only to turn our argument round. Hitherto we have argued from the number of bright stars how many faint ones there ought to be, and found the estimate deficient: if we start with the observed number of faint stars and calculate how many bright ones there should be we shall find them in excess, and the excess is due to the solar cluster. As an illustration, suppose we start with the number of stars of the 6th magnitude in the table given above and divide continually by 4, we get:

| Magnitude. | Number of Stars. | | Excess due to Solar Cluster. |
|---|---|---|---|
| | Calculated. | Observed. | |
| 6 | (5171) | 5171 | (0) |
| 5 | 1293 | 1740 | 447 |
| 4 | 323 | 506 | 183 |
| 3 | 81 | 157 | 76 |
| 2 | 20 | 52 | 32 |

and we have accordingly assigned 738 stars to the solar cluster. We have not much guidance as to the accuracy of this crude supposition but it is certainly well within the limits suggested by other clusters. On a photograph taken at the Yerkes Observa-

tory of the great cluster in Hercules, Mr. W. E. Plummer measured over 2,000 stars clearly belonging to the cluster; and just as this particular plate recorded more stars than others taken with inferior instruments, so a further improvement on the great Yerkes telescope would probably show an increase in the number of members of the cluster. There is every chance that the advance has already been made. Within the last few months we have seen the first results of the new 60-inch reflector of the Solar Observatory established by the Carnegie Institution on Mount Wilson, Cal., U.S.A. They are wonderful examples of what may be done in a really fine climate by a master in the construction and use of instruments. For the moment the latest photograph of the cluster in Hercules is not available. But we are indebted to Professor Ritchey for permission to reproduce his picture[1] of the globular cluster in *Canes Venatici*, which admirably illustrates our text. There is no reason why a single cluster should not contain millions of stars; from this point of

[1] See Frontispiece.

view, instead of the limits of our cluster being reached at about the 6th magnitude, there is no reason why they should not extend to the 7th, 8th or even much fainter magnitudes. But there are two considerations which make us hesitate to extend very far in this direction. The first is that by doing so we diminish the resemblance to other observed clusters in an important particular. In the clusters which we see in the sky the stars are thickest in the central portions. The law of condensation towards the centre has not been exactly formulated (there is room for an interesting research here): but something has been done, for instance, by Mr. Plummer in his paper [1] on the Hercules cluster; from the figures he gives we can infer that if an observer could be placed at the centre of the cluster to count the number of stars of successive magnitudes (as we have been doing for the stars visible from the earth), then the numbers would increase very slowly indeed. The ratio, instead of being 4 or 3, would probably be less than 2. Now, if we look at the numbers assigned to

[1] *Mon. Not. R.A.S.* lxv. p. 812.

our "solar cluster" by the crude supposition just made, we shall find that the ratio is greater than 2. We can reduce it by reducing the dimensions of the cluster (supposing the cluster to extend no further than the 5th magnitude, say) but the more we extend the cluster the greater we make this ratio. Hence this is, so far as it goes, a reason for moderation, though it must be admitted that the first argument is not very conclusive.

The second and more serious consideration is that, however far we extend the "solar cluster," we do not remove the chief difficulty. The ratio of the number of stars of any magnitude, to that of one magnitude brighter, obstinately refuses to rise up to 4, however far we count. The counting soon becomes very laborious, as may be seen from the figures already quoted: we have over 5,000 stars of the 6th magnitude, which means approximately 20,000 of the 7th, 80,000 of the 8th and so on; two more steps take us into the millions. It will cause no surprise that the counting has then to be done by inference from samples in different

parts of the sky, and is no longer complete, but even the imperfections of the counting fail to suggest any escape from the conclusion that the ratio is sensibly less than 4 Does then the "solar cluster" extend indefinitely? This would be only another way of saying that the whole universe is arranged with reference to our sun and its system A few centuries ago it was natural to put ourselves at the centre of all things and to regard the universe as a mere appendage but we have outgrown this instinct and we now feel suspicious of any suggestion which assigns special importance to our own position The evidence of the star counts is very striking but before accepting it as conclusive we feel bound to inquire whether it may not be susceptible of another interpretation

One such interpretation at least is open to us, and our familiar experiences in a fog are enough to suggest it We know how a moderate fog limits our visible universe in all directions in front, behind, to the right, to the left, upwards—downwards the earth anticipates the limit but from a balloon the

exception would be removed—in all directions there seems to come an end to our surroundings at about the same distance If we move about, objects appear suddenly within this charmed circle in front and leave it as suddenly behind us Were it not for our independent knowledge, we might believe that we were the centre of all things as it is, we attribute the appearance of centrality to the fog Even if the fog were not in other ways obvious—if, for instance, it were night time and the fog were too thin to irritate our nostrils—we might infer its existence from the fact that the street lamps seemed only to extend to a certain distance, instead of being visible indefinitely

A closely similar explanation can be given of the appearance of centrality suggested by the star counts the universe may be filled with a slight fog It must, of course, be so extremely tenuous that the name fog is completely unsuitable, for that name suggests to us something which quenches light very rapidly, so that within a few yards (sometimes within a few inches) the brightness of a light would be reduced to one half

The "fog" in space must require at least thousands of billions of miles to effect the same reduction to one half The size of these figures does not mean that they are hopelessly vague indeed we are almost in a position to say that the number of thousands of billions must be greater than 4 and less than 40, for various independent discussions of this most important matter have been made recently, and they all point to figures within the limits just specified

From the star counts alone we could not infer the existence of this light extinguishing medium, which we may continue to call a "fog" for brevity At any rate the alternative of a limited universe would have equal claims to consideration But the evidence for the fog has been steadily growing In the first place we have had before our eyes for centuries the spectacle of finely divided matter being driven off into space—in comets' tails and in the sun's corona There are various interpretations of both these phenomena, but the facts cannot be accounted for completely without some hypothesis of the escape of matter into space Again, it has

been realised that particles must be continually escaping from planetary atmospheres such as our own There is, in fact, no doubt of the existence of matter in the spaces between the stars the only question is as to its amount And, as a second line of evidence, the spectroscope seems to indicate that the amount is appreciable Professor Newall of Cambridge was so much impressed with the accumulated evidence of the spectroscope that he devoted to the subject a special Presidential Address to the Royal Astronomical Society in February 1909 "Here, then," he summarised, "are a few reasons for looking into possible practical ways of justifying the belief that in space, especially in the neighbourhood of suns, there must exist matter forming extended atmospheres" The phrasing is evidently that of a cautious reasoner, those who care to read the whole address will find ample confirmation of the suggestion that it is no idle speculation that is being put before us but a conclusion towards which we are urged from more than one side Thirdly, there is a direct test for the existence of a

fog which has been applied to the depths of space with apparent success. We all know that the sun looks red in a fog, because the red rays of light can penetrate a fog better than the more refrangible blue rays. For a similar reason our electric lights, being bluer than gas, suffer obscuration more readily in a fog. If, then, there be a fog in space, the more distant stars ought to appear redder than the nearer. The test, however, is not so easily applied to faint objects: for one thing we lose the sense of colour when the light is very faint. But there is one characteristic of red light that is familiar to all photographers: it takes longer to photograph it, unless we use special plates. We have then merely to ask the question, does the exposure required for the more distant stars increase in an unexpected way? The answer is certainly in the affirmative, though we must be careful that there is not another possible interpretation. From the very beginning of the work on the Great Star Map it has been a serious and fundamental difficulty that, when the exposure was doubled, the gain of faint stars on the

plate was not so great as visual observations would lead us to expect. The expectation was founded on laboratory experiments, which show that, within proper limits, a light half as bright as another will give the same photographic effect if the exposure is doubled. "Within proper limits"—here is the need for care: the law breaks down when the light is very faint indeed and we must be careful not to mistake a breakdown from this cause for a cosmical phenomenon. The "proper limits" are still under investigation, but they have already been subjected to careful scrutiny: a considerable research by Dr. C. E. K. Mees and Mr. S. E. Sheppard (to quote a single instance) indicates that the limit is reached, for such plates as are used in the Great Star Map, at about fifteen minutes of exposure.[1] Now well within this limit—for exposures of a few minutes only—we find that the difficulty of photographing faint stars is out of proportion to our visual expectations; and it is a fair conclusion that the difficulty arises from the characteristic

[1] See *Investigations on the Theory of the Photographic Process* (Longmans), p. 214.

property of a fog There is room for difference of opinion as to the intensity of the fog, for the observations are difficult to interpret and even treacherous but two separate discussions indicate as rough limits the figures which were given above A discussion by the present writer,[1] assigning the whole of the difficulty to the fog and thus probably giving a maximum density to it, made it extinguish half the light in about 4,000 billion miles A more conservative estimate by Professor Kapteyn of Groningen, who allowed for other possible contributing causes, makes the density about one-tenth as great We must hope that further research will narrow the trail but it will be surprising indeed if we find that we are altogether on a false scent

This rather long digression has not taken us so far from the topic immediately concerning us as might at first appear Without some such explanation it would not have been easy to realise the importance of mere counts of the number of star images of a certain size They might have been regarded

[1] *Mon Not R A S* lxix p 61

as of academic interest merely, whereas we now see that they furnish evidence on two fundamental questions firstly, is our Sun merely an individual star or is it associated with other stars in a family or cluster? secondly, is there an extremely tenuous "fog" of matter pervading the spaces between the stars and if so what is its density? We shall find that the first of these questions is presented again in another connection when we come to the movements of the stars but the existence of some sort of solar cluster is established by simple numeration combined with measures of brightness

How are we to measure the brightness of stars photographically? In approaching any measurement of differences we must first satisfy ourselves that we can recognise equality Let us define as of equal photographic magnitude two stars which impress the same plate equally in the same time and we need go no further to encounter trouble Suppose we pick out two stars by this rule, will they remain equal if we substitute a different plate? The answer is in

the negative for if one of the stars be a red star and the first plate be isochromatic, we shall find the image of the red star much fainter on substituting an ordinary plate If our photographic magnitudes are to mean anything, we must keep to the same kind of plate Strict uniformity in this respect has not been possible the sensitising of films is an art rather than a science, and when a firm of plate makers changes its artist, the plates undoubtedly change in character But it is hoped that the variations in character of plate throughout the work have not been serious enough to introduce large errors, though this is a point on which our information is not very complete Moreover this is not the only trouble. Two stars showing similar photographic images on the same plate may be made to give dissimilar images by slightly turning the telescope so that the images fall on different points of the plate or by refocussing the telescope The apparent photographic magnitude may thus depend upon the distance of the star from the plate centre and upon the particular focus selected for the

plate  The difference is so slight that it might escape detection by the direct process of comparing images  but it can be made unmistakably manifest in a very simple way  It has been already mentioned that the plates of the Star Map are ruled with a series of cross lines, called a reseau, dividing up the plate into equal squares  Let us count the number of star images in each of these squares for a large number of plates and add together all the counts for each particular square  then if stars photograph equally well all over the plate, the total numbers for each particular square ought to tend to equality  the stars are scattered sufficiently at random for this purpose  Now it is found that on the plates of the Star Map the numbers do *not* tend to equality  at the edges of the plate, the totals per square are considerably less than nearer the centre—the inequality may be as great as 1 to 2  The increase, however, is not maintained up to the centre  it reaches a maximum and then falls off again, unless the plate happen to be focussed in such a position that the centre is most favoured  The phenomenon depends,

in fact, on the focussing of the plate if it be focussed for the centre, then the total per square will be greatest at the centre and will fall off steadily towards the edges, but it is customary to push the plate a little further in than this, so that the region of best focus is a ring intermediate between the centre and the edges, and on this ring the total per square is greatest, falling off both towards the centre and towards the edges Photographers accustomed to ordinary cameras will read these words with some surprise, for they may not have noticed in their experience any corresponding phenomenon, but the reason of this is simply that they use a different kind of lens—a "doublet" made up of two lenses separated by an interval, and with this combination the trouble does not occur In sketching the early history of the Star Map it was pointed out that the selection of a lens was one of the important decisions taken by the Conference of 1887, and that it was decided not to use a doublet lens—chiefly because of the expense But in other connections stars have been photographed with doublet lenses

and it has then been found that the inequality of distribution of images disappears or is very considerably reduced  With the lenses used for the Star Map, however, the inequality is marked  By noticing where the total per-square is a maximum we can ascertain to a nicety how the plate has been focussed and whether the focussing has been changed from one time to another  The position of the maximum changes slightly with the season of the year, doubtless owing to the expansion and contraction of the lens and the tube with variations of temperature  We can even tell whether the plate is tilted slightly to one side, for then the position of maximum will be further from the centre on one side than on the opposite

Hence it will be seen that the determination of the photographic magnitudes of the stars is beset with difficulties from the outset  We must take into account the kind of plate used and the position of the star on the plate if we are to get comparable and accurate results  Nevertheless much information can be obtained by very simple means if these essentials are attended to

Suppose we take two plates from the same batch and expose them for the same time on the same night, one to a region in the Milky Way and the other to a region far from it and that we then count the total number of stars upon each. There will be many more on the former [1] and we can find definitely what the ratio of the two numbers is. Repeating the comparison with a longer exposure (still the same for both regions), we shall get another ratio. From the study of these different ratios for different lengths of exposure, we get information bearing directly upon the two great questions we have been studying—the existence of a solar cluster and of fog in space, for we must ascertain whether the Milky Way is in any way related to these possibilities. Hence it will be understood why counting all the stars on a plate, trivial as it might appear at first sight, has been an important operation in connection with the Star Map. It is usually done with a "billiard marker." Those who play billiards have various devices for marking the score, one of which

[1] See Note V.

is a small apparatus, held in the hand, provided with two or four little springs On pressing these springs the numbers shown on the face of the apparatus change by a unit and by a series of clicks the score is registered as required Now one of the astronomers who was taking a share in the chart had "misspent his youth," and accordingly knew of this apparatus and saw that it would be useful for counting star photographs, for the plate can be passed in review under a microscope with one hand, while with the other hand a click can be made (without removing the eye from the microscope) whenever a star is seen The success of the performance has been sufficient to cause "billiard markers" to be exported to distant astronomical observatories, where there is, as a matter of fact, no billiard table

But how comes it that different exposures are given in the actual course of the work on the Star Map? Is not uniformity one of the essential features of the scheme? It was certainly the original idea that a particular length of exposure should be selected and adhered to throughout fifteen minutes

found most favour. It was considered, in the light of the experience available, that fifteen minutes would give stars as faint as the 14th magnitude, and was not so inconveniently long as to be a tax on the observer. After this had been practically settled it was remarked that the bright stars would, with so long an exposure, form very large images, the centre of which could not be accurately determined. Now it is important to measure these bright stars accurately, for we already know their positions in the sky with some precision, and they are therefore useful reference marks for the others. Hence it was decided to take another series of plates with a shorter exposure (ultimately fixed at six minutes), on which the images would not be so large. This bifurcation of the enterprise was subsequently developed in both prongs. To the shorter exposure two others shorter still, of 3 minutes and 20 seconds, were afterwards added, for good reasons which we need not stop to notice; and the longer exposure of 15 minutes was extended to 20 minutes, then to 30 minutes, then to 40 minutes, and ultimately to one hour,

subdivided into three separate exposures each of 20 minutes We need not follow the reasons in detail, it is sufficient to remark that for the originally projected uniform exposure of 15 minutes a series has been substituted, for reasons more or less good, of four or five exposures ranging from 20 seconds to 40 minutes The ratio of 120 to 1 between the longest and shortest corresponds roughly to 7 stellar magnitudes, and hence we have material for studying the variation of total number of stars per plate over a considerable range

The particular proposal to give three separate exposures of 20 minutes each has had an unexpected result of an interesting kind The three exposures are arranged in the form of a little triangle, so that each star is represented by three little dots in this figure Now these long exposure plates are not to be measured but are intended for reproduction as charts They represent the most expensive part of the project, each of the eighteen shares costing some £10,000 in all At Oxford and at some of the other less wealthy observatories it has not been

possible to undertake this expensive part of the work, but the French are particularly interested in it, as a consequence of earlier national projects[1] of the same kind, and the French Government has accordingly undertaken the reproduction of the "Charts" in the Oxford zone and in some others These charts are being very beautifully and carefully reproduced in Paris by heliogravure As each chart is printed it is patiently examined for possible defects, to see for instance whether any of the images on the original plate are missing or whether any accidental blots could be mistaken for stars One test applied by M Jules Baillaud, who has special charge of this work, is a comparison of the three separate images of each star to see that they resemble each other, if they do not there is usually some defect in the reproduction calling for correction But what was his surprise one day to find a distinct difference in the images of a particular star which was not a fault of reproduction, but was apparent in the original plate itself! The conclusion was forced upon

[1] See p 8

him that the star had varied in brightness between the exposures The interval is usually so short that such an occurrence would be startling, but in this particular case there had been, owing to an accident, a longer interval than usual After two exposures had been successfully given, clouds had come up, and it was impossible to give the third on that night But in these days of "dry plates" such an occurrence does not mean the loss of the work already done, it was only necessary to close up the plate securely until the next fine night, when the telescope was again pointed in precisely the right position and the third exposure given But meantime the star had changed in brightness a little, and so M Baillaud's careful scrutiny enabled him to discover that it was variable—a success which he afterwards repeated in another instance Indeed the accident was suggestive, just as an accidental effect at rehearsal is often deliberately adopted subsequently in the play itself, so it was made clear by the unavoidable separation of the exposures in question that there would be a distinct advantage in separa-

ting them deliberately M Baillaud made this suggestion at the recent meeting of the Permanent Committee and the wisdom of it was at once recognised

The detection of variable stars, however, is not a regular part of the work on the Map These objects are few and exceptional we are concerned chiefly with the many and the average The many are counted by thousands and millions, thousands of stars on a single plate and millions on the whole collection of plates Perhaps a few definite figures may be given here not too many of them, for they are apt to be tiresome, but one or two representative figures will give a crisper idea of the magnitude of the work

There are eighteen observatories[1] concerned the share of each is about 1,200 plates, taken twice over with short and long exposures Fixing our attention on the short exposures, there are on the average 400 to 500 stars on each, the places of which are to be measured and recorded But this average is struck from numbers which diverge

[1] See Note III.

widely; on some plates there may be 5,000, on others less than 100, the rich plates being of regions in the Milky Way and the poor ones of regions far from it. Each observatory has thus to measure about half a million star images, and as the number of figures required to record each measure may be several dozen, it is easily seen that many millions of figures are used by each observatory. These measures took a staff of four or five people at Oxford some ten years or so to complete; and the printing of them another four years. The checking of so many figures in the proof sheets is no trivial matter, since it is important to avoid mistakes—astronomers know the trouble which may be caused by a wrong figure, once it gets into print.

In dealing with so many figures it is important to devise tests of their accuracy. In mathematical calculations, a whole series of operations can sometimes be tested from a single result; if that is correct, there cannot be a mistake in any of the operations, at any rate not one mistake alone; there may be two or more which exactly

compensate one another, but this risk is more remote. Just so in money accounts, if the totals check one another, it is usually fair to assume that the individual items are correct. In the case of the Star Map no such economical tests are possible, for no connection between the positions of the stars is known to us. We must be content to check each star by itself, and for this purpose two measures are made of it under different conditions. After all the stars on a plate have been measured, the plate is taken out of the measuring apparatus, turned round through 180° and put in again for remeasurement. The second set of measures gives an independent check on the first; in this way nearly all the unintentional slips are detected; so that when the results are printed and compared with both sets of measures, they are substantially correct. This is proved in the following ways: according to the decisions of the Conference the plates of each series are to cover the sky twice, with a certain overlap of adjacent plates, so that every star appears on at least two plates. After the measures have been

printed off the two independent measures for each star have been compared in many thousands of instances, the number of errors found is remarkably small Again, some of the plates accepted in the first instance did not come up to later standards and were repeated here again comparisons between old and new measures have detected a few mistakes, but not many These checks have given confidence in the general accuracy of the work

# III

# STAR POSITIONS

In the last chapter it was shown that from the mere counting of the stars (taking note of their brightness) it is possible to infer some important facts about the nature of the universe in which we are placed such as the existence of a cluster of stars to which our Sun belongs, and the existence of an extremely tenuous "fog" in the depths of space But we can always learn much more from prolonged inspection than from a mere glance A single photograph of a scene tells us only the actual situation at a given moment It may suggest that changes are going on, but to be sure of these changes we must have another photograph taken later, and if we can get a whole series taken consecutively, as in a kinematograph, we trace completely the history of the changes

The Great Star Map in process of construction is only the first picture, others must follow it if we are to study the motions of the stars, and our knowledge will grow with each repetition What revelations the future may have in store for us we cannot at present even guess, though it is not too soon to be learning something The important point to be remembered—and its importance can not be too strongly emphasised—is that the main purpose of the present project is to provide a basis for these future discoveries, by fixing the present places of the stars with such accuracy that movements can be detected readily It is only by keeping this fact in mind that we can understand the reasons for the great labour which is being undertaken so cheerfully A much less laborious project would tell us a great deal, thus nearly all the knowledge about the number of the stars of a given magnitude, which we considered in the last article, could be gathered from photographs taken on a very much smaller scale There is for instance a very handy map of the complete sky published by the Harvard University Observa

tory, consisting in all of only fifty five glass plates, each 10 in × 12 in, the whole weight of which is only about 30 lb, and the price $15 (rather under cost, owing to the liberality of the Observatory) When we compare with this the 22,154 plates of the Great Star Map, weighing 3 tons, or the 2 tons of paper which the chart reproductions will represent,[1] it is clear that the discrepancy needs explanation The explanation is simply that the "Harvard Sky," as it is called, though it will tell us many things, will not allow us to study the changes of the stars *in position*, because the scale is too small Other changes can be studied with its aid thus the magnitudes of the stars are shown by it rather better than on the Great Star Map, being more uniform in different parts of the plate, and we can study changes in magnitude by comparing two plates taken at different times The Harvard Sky is actually being used in this way to discover new variable stars, and a great many discoveries have already been made A positive copy of one plate is super-

[1] See Note VI

osed on a negative of the same region taken n a different date, and the sharp eyes of hree experienced ladies detect any want of orrespondence between the pairs of images t the end of the year 1909 the examination f twenty one out of the fifty five portions f the whole sky had yielded 211 new vari bles, and the efficiency of the search, and f the plates of the Harvard Sky as a means f conducting it, is constantly attested by the ediscovery of already known variables in he course of the examination If changes n *brightness* of the stars were all that need oncern us we need have no map larger in cale than the Harvard Sky, though it might till be profitable to use a more powerful nstrument so as to show fainter stars

But it is important, it is indeed of the ery greatest importance, to measure also hanges in the *positions* of the stars, for his purpose the Harvard Sky is unsuitable ecause of its small scale The places are orrect so far as they go, but the residual ncertainty is more than ten times that of the Great Star Map Consequently, to measure any given change of position we

should have to wait at least ten times as long, and since the majority of the changes with which we are concerned may be expected to require a century or more for their complete identification on the plates of the Map as planned in 1887, those who made the plans cannot be accused of extravagance or hurry.

The scale of a star map depends essentially on the length of the telescope used in making it. The little instrument with which the Harvard Sky was made is only about a foot long, whilst those used for the Great Star Map are about 11 feet. The surface of the representation increases, of course, as the square of the linear dimensions, so that we might fairly expect the weight of the plates to be increased in the ratio 121 to 1, and since the plans for the Map involve covering the whole sky twice over, we must double this, getting 242. Now the ratio of 8 tons to 80 lb. is 224 to 1, so that the weight is adequately explained by the increase in scale. If we use the same factor (242 or thereabouts) to get an idea of the cost of the plates forming the large map, from the Harvard $15 (which however is

less than the actual cost of the Harv Sky, though it is generously offered for at this figure), multiplying 15 by 242 get 8,600 dollars or £700 But the actual is greater than this because plate glass been used and two edges of each plate h been specially ground, probably £1,50 £2,000 is not too much to put down as cost of the plates But this after all is a very small portion of the real cost of Map, which arises chiefly from the done on the plates after they have taken In order to expedite compar with other plates to be taken in the fu the present positions of the star im on the plates are being carefully measure the measures printed How much sha set down for this? It is impossible to an accurate estimate since circumsta vary so much with nationality, but rough idea may be gathered from th perience of Oxford, where the measures been made and printed Even here are difficulties, as will appear from th lowing statement of a generously sup limit to the total cost

TOTAL POSSIBLE COST OF THE OXFORD ASTROGRAPHIC CATALOGUE

| | £ |
|---|---|
| Telescope given by Dr Warren De la Rue | 600 |
| Maintenance of University Observatory for twenty years including Assistance | 13 000 |
| Salary of Professor for twenty years | 18,000 |
| Government Grant (from the fund administered by Royal Society) in the years 1896–1910 | 1 200 |
| Printing (shared between the Government and the University) | 1,200 |
| Total | 34 000 |

The chief difficulty is to determine how much of the two main items, the maintenance of the Observatory and the salary of the Director, are to be credited to this particular work In the above statement the whole of the Professor's salary has been set down, not even deducting income tax, but some of it must be credited to teaching and other duties which fall on him as on his colleagues

The same difficulty arises in a smaller degree about the second item in the list, as the assistants have by no means confined themselves entirely to work on the star map As the best estimate possible under the circumstances we may take £20,000 perhaps

as the Oxford contribution to this great work, and as there are seventeen other contributors, many of whom are not working under such favourable conditions, the total cost will be at least half a million sterling

These estimates have not been made and quoted for sensational purposes but with the very definite object of showing the necessity for care in procedure We are dealing with big figures—long periods of time and large sums of money We must on the one hand spend money freely to save time we must make a map on a large scale, so that we may determine the movements of the stars within a reasonable period On the other hand, since every detail of the process will be repeated thousands or even millions of times, we must be extremely careful in settling the details so as to save expense If one figure will suffice rather than two, the difference may seem trivial in a single instance, but when multiplied a million times will constitute a serious item of unnecessary expenditure If one measure will give fair accuracy, then before yielding to the temptation to increase the accuracy

a little by making a second measure, we must remember that we are multiplying the total cost of measuring by two, and consider carefully whether the extra expense is justified, and that we are also multiplying the time required for completing the work by two, and consider carefully whether the completion can be so long delayed without serious disadvantage When confronted by such problems, the different contributors to the scheme have naturally differed in their solutions At Oxford we have throughout fixed our attention on getting the work done as quickly and economically as possible, consistently with certain rules laid down by the International Committee, nevertheless the work has taken twenty years Of course with a larger staff the time might have been shortened, and at Greenwich, where the available resources are greater, a more elaborate programme has been completed in a time shorter by a year or so but nowhere else is the work yet finished, the prospects of completion being in many cases very remote, and seeing that at the outset five or ten years was mentioned as the proper

time for the work, the present situation gives some cause for anxiety[1] The fact is that the necessity for strenuous economy in detail has not been sufficiently realised some of the larger observatories strained at an accuracy scarcely possible even for them, and their weaker brethren, in attempting to copy their example, have been left far behind Moderation and self denial are just as necessary in astronomical work as in other walks of life

Let us consider in detail the nature of the work to be undertaken The process of measurement of the positions of the star images on the photographic plates has been much facilitated, as already mentioned, by the impression of a *reseau* on the plates Two series of equally spaced lines are ruled, one set at right angles to the other, so that the plate is divided up into a number of small squares of exactly the same size It is necessary to specify what is meant by "exactly" in this connection Nothing, of course, is really perfect or exact, but for practical purposes we may regard a ruling

[1] See Note X

as exact if the errors are so small as to be negligible in comparison with the accidental errors of measurement. In this sense and for the purposes of the Map we may regard the little *reseau* squares as accurate and exactly equal. If we number them from left to right ($x$) and also from below to above ($y$), then two appropriate numbers ($x$ and $y$) will specify the square in which any star image lies; and if in addition we measure the distances of the image from the sides of the square, we shall complete the specification of its exact position. The distances are expressed (in the decimal notation) as fractions of the side of a square and written down immediately after the whole numbers specifying the square. There is no difficulty about the whole numbers: the doubtful points all arise in connection with the fractions. It would take too long to consider them all; we take two important ones as illustrations.

Firstly, how often should we recur to the image of a single star? We have to measure two co-ordinates, $x$ and $y$; we may repeat the measures of each: we may do this for

each of the two or three images which occur on the plate (according to the plan already explained) and take the mean, and we may then turn the plate round into another position and repeat the measures. (This last precaution will need no explanation to any one who has had experience of such measurement: it detects and eliminates well-known personal peculiarities in the measurer.) It would therefore be easy to adopt a plan of measurement which would involve recurring to the same star $2 \times 2 \times 3 \times 2 = 24$ times, without real superfluity. Indeed, such a process would be definitely advisable for a small piece of work wherein the utmost accuracy was desired. But what we have to settle with regard to the project before us is whether we can afford it. For comparison let us take the minimum instead of the maximum advisable programme. We can measure both co-ordinates of the star at a single setting on a single image, and this would be the actual minimum, but scarcely advisable—for there is nothing to check a mistake. To check mistakes we must have at least another measure, and if we turn

the plate round through 180° to make this, we shall at the same time eliminate the personal errors referred to above This, then, may be taken as the minimum advisable programme, it involves recurring to the same star twice and twice only It was adopted at Oxford, and the work of measurement took even then a dozen years it will be seen how easily this might have been turned into half a century or more

The second important question of detail concerns the apparatus for measuring the fractions of a square That which first occurred to the astronomer was the micrometer screw, with which he was already familiar in work at the telescope, at many observatories this type of instrument has been adopted for use A spider line in the microscope is set on the side of the *reseau* square and the reading of the micrometer screw noted then the screw is turned until the spider line falls on the star image and the reading noted again finally the screw is turned further until the spider line falls on the opposite side of the *reseau* square and the reading noted once more From these

three readings and a little arithmetic the quantity required is deduced This process can be made very accurate, though there are some difficulties, especially those resulting from gradual wear in the screw when it is used thousands of times But though accurate, it is very slow It is far quicker to abolish the screw and substitute a finely divided scale in the field of view of the microscope It has been shown that the fractions can in this way be read off at sight without losing time in turning a screw The rapidity of the process naturally excited suspicion at first that it might be too rough, in order to combat this prejudice, one of the advocates of the new method took over his apparatus to Paris on the occasion of the assembly of 1896, and offered to give a demonstration A committee was appointed to sit upon him they shut him in a room with his machine and a photograph he had never seen before, he was to produce as many measures as he could in half an hour At the precise second completing the thirty minutes the door was opened and his measures impounded It was found that the

prisoner had measured twenty five stars with satisfactory accuracy, and by many this demonstration of the qualities of the machine was accepted as sufficient. With experience a still greater pace can be acquired, but we may take fifty stars per hour as a fair average rate for one person (though two people working together can do better). Now it is easy to spend two or three hours on the same fifty stars if we use a screw instead of a scale, so that here again we have a danger of unduly prolonging the work.

The view here expressed that it is stringently necessary to study economy of time and labour is frankly that of an advocate. On the other side there is considerable weight of tradition and opinion which remains unshaken even by such consequences as the great delay in completing what was originally intended to take ten years. Our scientific traditions have come down to us from times when workers were so few and scattered that almost anything they produced, however planned, was precious if not priceless; we see the consequences of this early practice in some huge editions of the

correspondence of great scientific workers in which nothing is too commonplace to be included It is quite possible that considerations of economy have been rightly disregarded in dealing with this sacred past, but does this justify a similar attitude with regard to the future which is better under our control? Scientific workers are no longer few and isolated, they are numerous and they are binding themselves into organisations (among which that for making the Great Star Map has an early and an honourable place) It does not seem unreasonable that the changed conditions should leave their mark on the methods of work, and that the relation of cost to value of product should be considered as in other enterprises In old days the value of the product was so high that any cost could be neglected, and there are still cases where this is the correct view—let us hope there always will be But in the case of a great piece of straightforward measurement like the Star Map, the value can be expressed very definitely by the "probable error" of the result, and alternative plans for the work can be

compared by setting down the probable error and the cost (in time and money) of each. So far as I know, however, this principle has not yet been applied except in a special case in geodesy. It is certainly one that may be applied in other sciences if judged sound; but at present it scarcely seems to have met with the approval, even the attention, of any large body of scientific workers. I trust no apology is needed for inviting attention to it at the present juncture, even at the expense of a slight digression.

Let us now consider what is to be the outcome of this immense piece of work. What are we likely to learn from these millions of measurements? As already stated, the interest will come when it is repeated—in the study of the movements of the stars, which are so minute that, as a rule, at least a century is required to discern them even by our improved modern methods. The movements are not really slow; we may take the velocity of our earth in its annual journey round the sun—about 20 miles a second—as a fair sample of the velocities of the stars. But our great

journey from side to side of the sun (nearly 200,000,000 miles across) would seem a minute movement to the nearest star, and to the great majority would be imperceptible This is however not the only movement of the earth, the sun himself is moving and we partake of that motion also It is not a circling or oscillatory motion, but is in the same direction year after year, so far as we can at present ascertain, the distance traversed each year being about 400,000,000 miles One year's journey is therefore scarcely more perceptible from the distant stars than the circling movement of the earth; but as year follows year the successive steps add together, and the cumulative effect becomes ultimately perceptible even to very distant stars Now all the stars are moving in this way—persistently in the same direction—year after year Hence, though the movement in one year may be imperceptible, by waiting ten years or twenty or a century we ultimately perceive the movements of many of them The more distant require even longer—how much longer we cannot yet say, this is one of the questions on

which we hope to get some light by the work on the Great Star Map and its successors. Our knowledge will grow: we shall find that after ten years a certain percentage of the stars have moved; after twenty, new movements previously undetected or uncertain, will be added; after thirty, more still, and so on; and by watching the run of the sequence we may even be able to predict what will happen in longer periods not yet reached, though this extrapolation has its risks.

We cannot, of course, afford to repeat the whole map every ten years; we must be satisfied with samples made as representative as possible. We have already obtained some samples at Oxford, and the following results will serve to illustrate our expectation. Plates have been repeated after intervals varying from ten years to seventeen and the measures compared. If the measures of any star in either co-ordinate differed by more than 1″.2 (the angle subtended by an inch at a distance of $2\frac{1}{2}$ miles) they were carefully repeated. In a number of cases the discordance was found to be due to

some mistake or a careless measure (It must be remembered that many thousands of measures were made in all, and that occasional mistakes are inevitable) But in the majority the difference was confirmed as due to a real movement of the star Such movements were nevertheless very rare—on the average less than 2 per cent of all the stars examined

The percentage was higher for the longer intervals somewhat as follows

| | | | |
|---|---|---|---|
| After | 10 years | 1 per cent | had moved appreciably |
| ,, | 12 ,, | 1½ | |
| ,, | 14 | 2 | |
| | 16 | 2½ | |

But this method of statement is defective by reason of a most significant fact brought to light by the results themselves

Of the plates examined for these movements some were of regions in the Milky Way thickly populated with stars—say 500 or 600 on a plate, others were of regions remote from the Milky Way and contained only 50 or 60 [These numbers are smaller than they should be, that is why the plates were repeated and the comparison rendered

possible.] Now we might naturally expect to find more moving stars among the 500 than among the 50, but we do not; there are just as many per plate (that is to say on a given sky area), in any part of the sky, whether it be densely or sparsely covered with stars. It is as though a fall of snow had collected into huge irregular drifts and then a shower of rain fell: the number of raindrops falling on a given area would have nothing to do with the quantity of snow upon it already, and we should only make confusion by expressing the rain as a percentage of the snow on the area. Similarly we must give up the idea of finding so many sensible movements per cent. of the stars on a given area and think of the number per unit area, irrespective of the population by other stars. The moving stars are in fact distinct from the others in some way, and it is pretty clear that the distinction consists in their being considerably nearer to us. If this be the correct interpretation, we infer that the stars near us are scattered more or less uniformly and do not show the structure which is so striking a feature of

hose more distant This is a fact of fundanental importance and suggests from a new oint of view the idea of a solar cluster— group of stars (of which our sun is one) tanding somewhat apart from its distant urroundings—which was suggested by the ounts of stars of different magnitudes as described in the last article

And its importance is enhanced in consequence of a great discovery made within the last few years, that there are two great streams of stars meeting one another It was assumed until about 1904 that the movements of the stars were at random in all directions, in that year Professor Kapteyn of Groningen showed that this could not be the case,[1] but that there must be at least two main streams of stars passing one through the other This suggestion has been confirmed by elaborate investigations of Eddington, Dyson and others and in particular Professor Dyson (now Astronomer Royal) showed that this bifurcation was

[1] A few months later Mr H C Plummer independently pointed out the same fact (*Mon Not R A S* vol lxv p 568 )

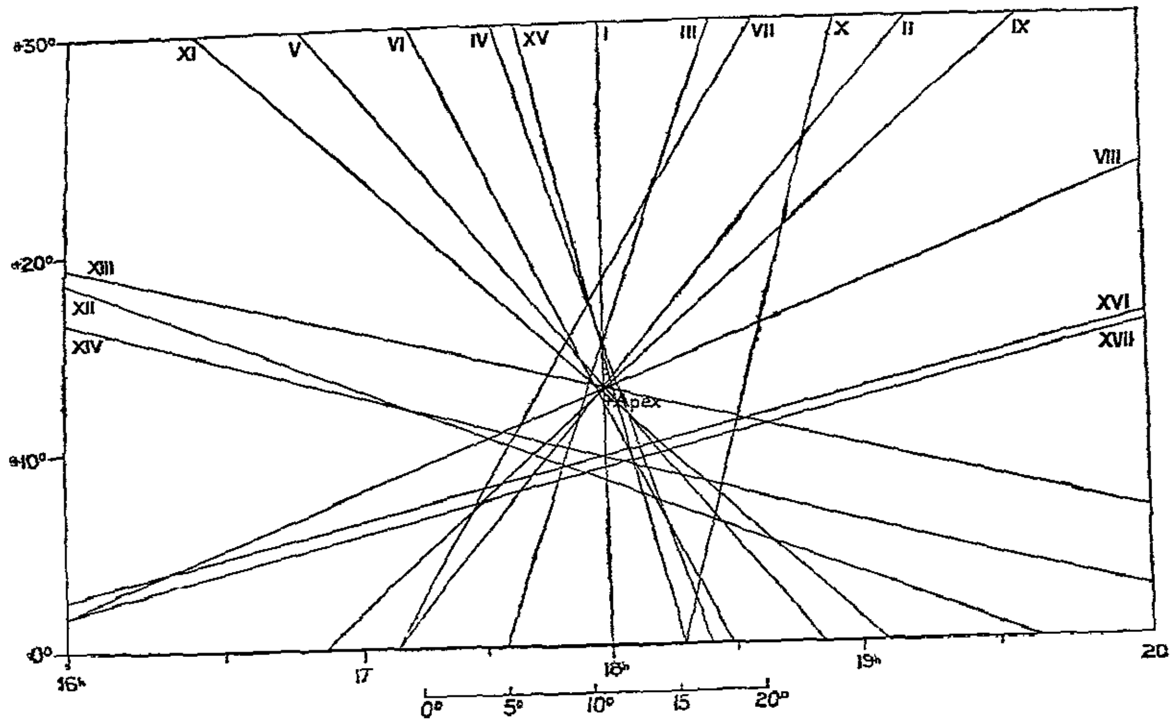
specially characteristic of the largest proper motions that is to say, the stars nearest to our sun are moving in this way in any case, whatever may be the real facts about the more distant stars of which our knowledge is still uncertain and incomplete Is there any reason for thinking that the bifurcation characterises *only* the nearest stars and ceases beyond? One such reason has already been indicated the nearest stars are apparently distinct from those more remote which cluster towards the Milky Way Hence the bifurcation must be proved independently for these remoter stars, since there is apparently a breach of continuity

But another reason has been suggested also In a most interesting lecture on the Milky Way, delivered to the British Association at its South African meeting,[1] Mr A R Hinks of Cambridge developed the idea that the Milky Way was made up of a number of independent star clouds or clusters If these are in relative motion, as they presumably are, there will be occasions on

[1] See an abstract in *Proc Camb Phil Soc* vol xiii Pt IV

ıch one cloud meets anotheı The staıs each being widely scatteıed, one will pass ough the otheı fıeely, without much ıısk collısıon between any of the membeıs ıs supposıtıon would explaın all the ıın facts as we know them at pıesent, t we cannot say how faı ıt will fit ın th facts to be dıscoveıed ın the futuıe, ıen we have compaıed plates taken at eateı ıntervals, and begın to leaın some-ıng of the movements of the moıe dıstant ııs

By the kındness of Mr Eddıngton (Chıef ssıstant at the Royal Obseıvatory, Gıeen ıch) I am enabled to ıepıoduce two dıa-ams whıch show the veıv latest pıece of ıdence ın favouı of the exıstence of these vo staı dıifts It should fiıst be pıemısed ıat sınce a seııes of paıallel lınes, such as ıe paıallel edges of a box, appear to us converge to a poınt (the "vanıshıng-poınt" [ peıspectıve), so a clusteı of staıs movıng ı paıallel paths, lıke a flock of mıgıatıng ııds, would seem to us to have movements onveıgıng to a definıte poınt ın the heavens beautıful ınstance of such conveıgence



+30°
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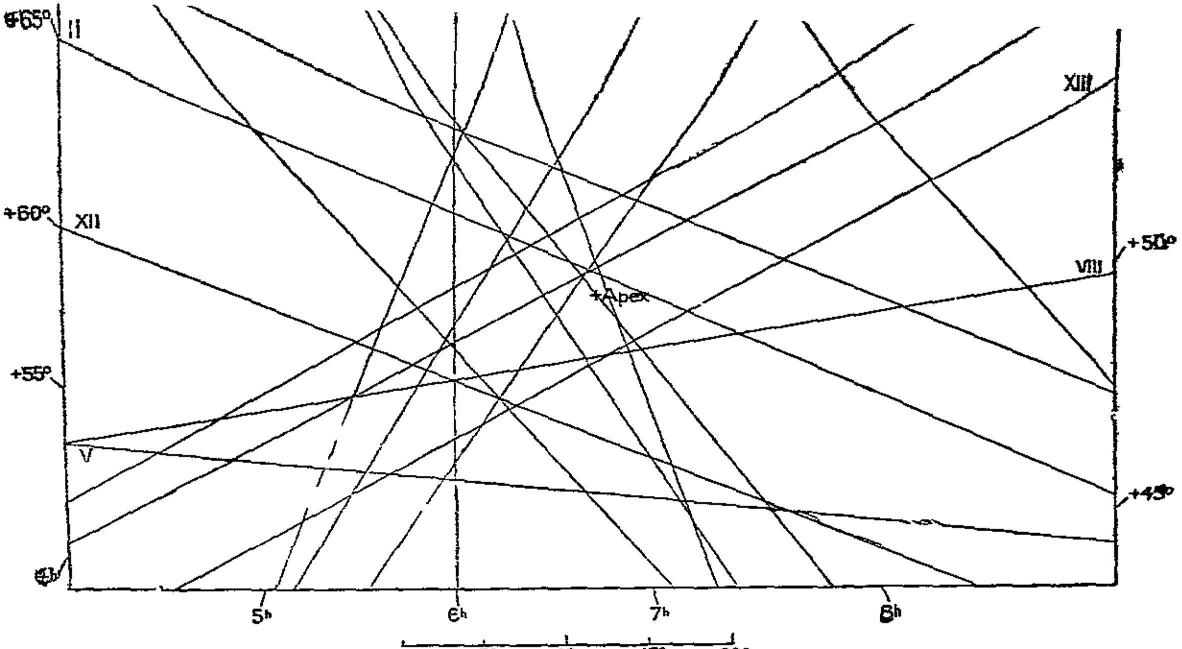



Mr Eddington's diagrams of the convergence of movements from seventeen pairs of regions covering the whole sky

NOTE —Each picture represents a portion of the celestial sphere corresponding nearly to that occupied by Europe on the terrestrial sphere.

among some stars in the constellation Taurus was detected a couple of years ago by Professor Boss of Albany, N.Y. He had suspected its existence for nearly twenty years, but the knowledge of the stellar motions was too inaccurate to convert his suspicion into certainty. This has only come with the completion of a vast research on the movements of the stars which he has conducted with infinite patience, removing one source of error after another by a laborious series of approximations until at last he was able to produce a catalogue of movements freed, as far as possible, from all discernible systematic errors. Incidentally he got values for the motions of the Taurus cluster sufficiently accurate to make it clear that they were apparently converging to a point. With this clue and the help of spectroscopic observations he was able to determine the distance of the cluster to be 120 light-years away from us (that is, light from the cluster takes 120 years to reach us; the distance in miles, if that be preferred, is 800 million million), and it is receding in an oblique direction. It passed us closest about 8,000

centuries ago, at about half its present distance; and he gathered further particulars of its history and shape, which we can scarcely stop to notice here. One further point, however, is of importance. The individual stars seem to keep their places in the procession without internal rearrangement—they move in almost strictly parallel lines and at the same pace.

Now the streams of stars to which Kapteyn called attention are of a different kind; the internal movements are considerable; it is only the average movement which is steadily in one direction. But when we take such average movements in different parts of the sky they tend to converge to a point like the actual motions of the individual stars of the Taurus cluster. It is this convergence of average movements which Mr. Eddington has represented so beautifully in his diagrams. He divided the whole sky into thirty-four areas, and found (from the great catalogue of movements just published by Professor Boss) the average movements in each area. It would take too long to explain how he identified the average movement for

each of the two drifts it must suffice that the process was ingenious and effective[1] He was able to draw the two lines for each of the thirty four regions, or rather for each of seventeen pairs which he preferred to use The test of the validity of the hypothesis is that these lines should converge to two points in the sky representing the goals towards which the two clouds of stars are drifting The reader can judge for himself In one case the convergence is very striking It is not, of course, perfect we could scarcely expect perfection when dealing with averages of imperfect observations, but the approximation is clearly a very close one In the other case the convergence is less marked, but the reality is brought home to us by an analogy "If from seventeen points," writes Mr Eddington, "distributed uniformly all over the earth, tracks (great circles) were drawn, every one of which passed across the Sahara, they might fairly be considered

[1] Those who care to read more will find Mr Eddington's paper in the number of the Monthly Notices of the Royal Astronomical Society for November 1910 It gives references to previous work

to show strong evidence of convergence the distribution of the 'drift II' directions is quite analogous"

We may note yet one more point in this very interesting paper Eddington found that there was a whole class of stars which it was better for him to exclude They seem to have a common motion of their own, like that of the Taurus cluster Moreover, their spectra are all alike (of the Orion type), which is further evidence of relationship, and finally they present two indications of great distance—first that their apparent movements are very small, and next that the stars themselves cluster towards the Milky Way (We have seen that the stars presumably near to us have large proper motions and are distributed indifferently) The inference that the distant stars forming the Milky Way do not share in Kapteyn's two drifts seems to be plain Recurring to Hinks's idea of star clouds, it seems probable that these "Orion" stars belong to a distant cloud, distinct from the two which have met and mingled in our neighbourhood But before we can accept these rough suggestions

as facts we must do much more work in the examination of stellar movements, such as it is the object of the promoters of the Great Star Map to initiate

One feature of such work on the stars which impresses itself deeply on the consciousness of those who undertake it is worthy of more than passing notice, though it may not be easy to communicate the impression to others In dealing with the comparison of the places of thousands of stars at two different epochs, a feeling of awe is evoked on finding so few cases of change As one turns over page after page of records and sees at a glance that the differences are too small to be significant, the first feeling of mere satisfaction at the accuracy of the measurement gradually yields to this growing sense of the profundity of the depths of space which makes this awful stillness It might not be suspected that pages of figures could serve to develop so sentimental an impression the layman would be prepared to learn that the observer of distant stars in a huge telescope might feel emotion, but figures, especially in a cataract of thou-

s, seem far too prosaic Nevertheless interpretation of the figures becomes practice a very rapid mental process, hat one sees behind them the realities indicate

long piece of work of this kind is indeed tive in condensing a number of mental esses Another illustration of a very rent kind may be given It has been ady explained that to guard against mis s each plate is measured twice over in rsed positions The two measures of star are represented by quite different res, connected by the rule that their sum t represent the whole width of the plate, )00 Thus, if the first measure be 8 352, second (in the reversed position) should 17 648, since the sum of these two num s makes 26 000 Now it will be seen that of these numbers can be derived from other by the following processes sub ct 8 from 25 and we get 17, subtract 3 l 5 each from 9 and we get 6 and 4, tract 2 from 10 and we get 8 This is traightforward but not very simple mental ration, which most of us would perform

for the first time with some wariness It fell to the lot of one of the computers at Oxford to perform it many thousands of times in reading proof sheets He presently became so adept that it was easier for him to read the derived figures than the direct ones! If set to read actual figures before him in the usual way, he would stumble, but allow him to transpose them as above and he proceeded with confidence and accuracy We know that pictures of external objects which fall on our retinas are inverted, and that nevertheless there is no consciousness of inversion in our perception of them, and this result has been ascribed (though not without misconception) to long habit It was, however, quite new to me to find that the mental process described above could be rendered automatic by the practice of a few months

## IV

## SOME INCIDENTS OF THE WORK

THE general history of this enterprise as exemplified more particularly in the portion of the work undertaken at Oxford has now been given and it remains to notice several incidental investigations of different kinds which have branched from the main project In a piece of work already extending over about twenty years, in a new department of science such as the application of photography to astronomy, it is only natural that the consequences of the departure should not have been foreseen in their entirety at the outset

The first novelty which attracted our attention at Oxford was what is called the "magnitude equation" of the Cambridge meridian observations In order to deter

mine completely the places of the stars on any one of our photographic plates, it was necessary to know the places of a few of them in the sky, so that we might virtually peg down the plate in its proper place on the sky and refer all the new and previously unmeasured stars to their proper positions For this purpose a large number of meridian observations made at the Cambridge Observatory some years before were ready to hand Having selected the stars required and used them without difficulty for the purpose described, we found what are called the "constants of the plates" For each plate two stars would have sufficed, had everything been theoretically perfect, but in order to compensate for the small errors of various kinds unavoidable in scientific work, it was desirable to make use of many more stars than the theoretical minimum From the general average of all the stars considered we ascertained the relative errors of individual stars, it was soon seen that a peculiarity was manifest in the individual errors depending upon the brightness of the star, and from independent information it

was known what was the reason of the discrepancy

The Cambridge observations had been made by watching the transit of a star across spider webs, recording the time of transit according to the clock It has long been known that different observers have a persistent personal characteristic which has been called their "personal equation," in virtue of which they are systematically a little early or a little late in their records More recently it has been found that even the same observer will vary in his habit according to the brightness of the star, the general tendency being to be late for the faint stars The tendency is more marked in some individuals than others The Cambridge observer (the late Mr A Graham) apparently had a strongly marked tendency of this kind

Various methods have been suggested for the valuation of this habit, especially the method which depends on using gauze screens to reduce the light of a star and thus to substitute for it a virtually fainter star occupying exactly the same space as the brighter

one If the observer were free from the magnitude equation error he would make a record of the transit precisely the same in the two cases, but if he be subject to the malady, his records will differ by an amount which affords a measure of his predisposition There are, however, some difficulties of a practical kind in using this method, and it was pleasant to realise that in the photographic plate we had found a simple and effective means of determining the magnitude equation without the necessity for any special observations on the part of the observer

A few details may be given which bring out some interesting points The first attempt at detecting the magnitude equation from the Oxford measures was made by Mr Hinks in 1897, when only seven plates had been measured (*Mon Not R A S* lvii p 473) The material available was only sufficient to demonstrate the value of the method, Mr Hinks recording his opinion that "when the reductions for the Astrographic Catalogue are completed, it will be possible to discuss very accurately the personal equations de-

pending on magnitude." The reductions are now completed and the discussion is being undertaken, but we did not wait until now for confirmation of the forecast. In 1899, when 600 plates had been measured, an examination was made of the accumulated measures (*Mon. Not.* lx. p. 8), the stars being grouped in half magnitudes, and it was found that the fainter stars had been "observed late" by Mr. Graham as follows, taking as standard those of magnitude 6.0.

DETERMINATION OF MR. GRAHAM'S MAGNITUDE EQUATION IN 1899

| | | | |
|---|---|---|---|
| | | | s |
| Magnitudes | 6.5 to 6.9 | 147 stars | 0.016 late |
| ,, | 7.0 to 7.4 | 320 ,, | 0.025 ,, |
| ,, | 7.5 to 7.9 | 604 ,, | 0.038 ,, |
| ,, | 8.0 to 8.4 | 572 ,, | 0.059 ,, |
| | 8.5 to 8.9, | 1226 | 0.086 ,, |
| | 9.0 to 9.4 | 2001 ,, | 0.146 ,, |

It will be noticed that there are roughly twice as many stars in the second group as in the first, and in the third twice as many again, this being the natural increase of stars in the sky as we go to fainter magnitudes. But at the fourth group there is a discontinuity, owing to the fact that only half the

available material was discussed beyond this point The labour was considerable, and it was thought that the examination, which was in any case only preliminary, need not be carried further at that time In the fifth and sixth groups the increase is resumed The discontinuity should not, of course, affect the averages in the last column, and we see that there is an increase of "lateness" which seems to be rapidly growing, since not only the quantities themselves, but their differences, get larger and larger There is no suggestion of a sudden jump, such as we shall notice in a moment, the smoothness of the growth was considered to be satisfactorily established and the matter was left there until the present time The measures are now printed and a final examination can be made, using the whole material and classifying the stars in smaller subdivisions, especially where they are numerous, as is the case for the fainter magnitudes The work is only in its early stages but already an important new fact has come to light, as will be seen from the following figures, from which the brighter stars have been omitted,

because hitherto only a few observations of them have been collected definitively

PROVISIONAL RESULTS FROM THE EXAMINATION OF 1911

| | | | |
|---|---|---|---|
| Magnitudes | 7.9 8.0 and 8.1 | 81 stars, | s<br>0.049 late |
| ,, | 8.2 8.3 and 8.4 | 102 ,, | 0.052 |
| | 8.5 8.6 and 8.7, | 197 | 0.073 |
| | 8.8 and 8.9 | 143 , | 0.085 |
| , | 9.0 | 259 , | 0.098 |
| , | 9.1 | 138 ,, | 0.155 |
| , | 9.2 | 124 | 0.170 |
| ,, | 9.3 | 130 , | 0.176 , |
| ,, | 9.4 | 81 | 0.178 |
| , | 9.5, | 154 | 0.191 |

The remarkable thing here is the sudden jump from magnitude 9.0 to 9.1, after which the further change is small. The actual difference in brightness of the star is so small that it is hard to believe that this discontinuity can have arisen naturally. It is possible that the observer had some special rule of procedure when he set out to observe a star catalogued as fainter than 9th magnitude—for instance, he may have arranged the illumination differently. The further investigation of this matter must be left until more stars have been examined, but

enough has been said probably to show the value of the photographic measures as a check on intricacies of personal equation.

A new enterprise of a more important and unforeseen kind arose from the discovery of the little planet Eros in 1898. Our solar system, which at the time when the days of the week were named contained only five planets in addition to the sun and moon, is now known to consist of many hundreds, and new members are being discovered almost weekly. Most of them are tiny rocks, probably not more than one or two hundred miles across, with no perceptible influence on the movement of their more important brothers and sisters; in fact, of no particular interest, as far as we can see at present. Astronomers were beginning to get rather tired of the continual discoveries of new small planets, which brought increased responsibility for keeping watch on them and increased labour in calculating their movements, without any obvious advantage from the increase in our knowledge.

It was therefore a distinctly sensational incident when one of these discoveries proved

to have a considerable importance, owing to the fact that the tiny object moved in an orbit which, in one part, was exceedingly close to the orbit of the earth The orbits of these small planets lie in general between those of Mars and Jupiter, and up to 1898 none of them had been suspected of approaching the earth nearer than the planet Mars But it was seen that the orbit of Eros lay within that of Mars and that only a few years previous to its discovery (namely, in 1894) the earth and Eros had been simultaneously in the adjacent portions of their orbits and had therefore been very close together

Now such a close approach affords an opportunity of a special kind for determining accurately the distance of the little planet from the earth Usually the planets are so far away that their distances are many hundreds of million miles, exceeding the diameter of our tiny earth so vastly that it is difficult to institute an exact comparison between the two, as we must if we wish to express the former in units familiar to us The difficulty is precisely the same as that which

we find in realising the distances of remote objects by the use of our eyes alone. There is no similar difficulty in perceiving the distances of objects close to us—say those within an ordinary room: they present different aspects to our two eyes and from these differences in aspect we are able to judge of the distances. But the change of aspect is smaller for objects more remote, and we know that it is entirely insensible for an object so remote as the moon. Indeed, our power of perceiving distance by means of the difference in aspect for our two eyes breaks down long before we reach the moon, although we do not always realise the breakdown, because other methods based on general experience frequently come to our aid.

In the same way, astronomers pointing telescopes from opposite sides of the earth to the same object can perceive its distance by a method similar to that we use unconsciously when we look at anything with our two eyes. But the observations become difficult when the object is too far away, and are only satisfactory for a comparatively close object.

The heavenly body closest to ourselves is of course the moon, and we know its distance within twenty miles Up till 1898 the next closest known were Mars and Venus, on favourable occasions, and accordingly much time and trouble have been spent in determining the distances of these two planets when there has been a Transit of Venus or a favourable Opposition of Mars

It should perhaps be remarked here that the *relative* distances of all the planets from the sun and from one another are known with great precision from the times which they take to describe their revolutions round the sun, so that when any one of them has been determined, we can obtain any other we please by a simple rule of three sum Or, to put the matter in another way, we can make an accurate map of our solar system, being in doubt only about the scale of miles which usually accompanies such a map Any single distance on the map being known, we could construct this scale and so find all the others Hence it did not much matter whether we determined the distance of Mars or of Venus or of any other planet

which might offer greater advantages than they; the new discovery of Eros offered just such greatly increased advantages. The opportunity is however not open always, but only at certain times and seasons. One particularly tempting opportunity had been unfortunately lost in 1894 owing to our ignorance of the planet's existence. But it was seen that another opportunity was coming in 1901, not so favourable as that of 1894 but still well worthy our attention. It may be added that the next good chance will not come till 1931, so that it is easy to understand the anxiety of astronomers to take advantage of the occasion of January 1901. They were, however, taken at a disadvantage by the comparatively short notice. There was no time to think of preparing special instruments; prudence suggested utilising such instruments as were already in good working order, and especially the battery of photographic telescopes engaged in making the Great Star Map. It needed a good reason to justify this diversion of their activities from the great work, which was alone sufficient to occupy their undivided

attention, but the reason which had presented itself so suddenly was felt to be good enough At the meeting at Paris, in 1900, of the Committee charged with the work on the map, the President (M Loewy) proposed that this digression should be made, and the proposal was unanimously adopted

Accordingly for some months during the winter 1900–1, most of the telescopes were withdrawn from the work on the map and were turned on the little planet Eros The chief aim of the programme was to take photographs as soon as possible after sunset and as late as possible before sunrise, for on these occasions the telescope would be as nearly as possible on opposite sides of the earth A few sentences ago we compared a pair of eyes to a pair of telescopes pointed at the same object from opposite sides of the earth, but a single telescope may be made to serve the purpose of a pair, since the rotation of the earth carries it round during the night from one side to the other, and this will explain the sunset and sunrise exposures to the planet Eros

In this way many hundreds of such photographs were obtained.

The next thing to do was to measure all these photographs accurately, in order to determine the place of the tiny planet among the stars. This place was of course changing continually, owing to the movement of the planet round the sun and indeed owing to a similar movement of the earth also. But it was possible to devise methods of allowing for this movement and correcting the measures for it. There would remain the displacement due to "parallax," that is to say, to the finite distance of the planet which it was required to measure. The greatest amount of such displacement was about 28″—about one hundredth part of the moon's apparent diameter, which it was desired to measure if possible to the thousandth part of itself. Hence the measurement required a new order of accuracy, the apparatus already in use for the Great Star Map needed modification in essential details for this new enterprise. Moreover it is a familiar fact in scientific work that, when we proceed to the next decimal place, we always encounter

a number of unforeseen difficulties of all kinds, and the measurement of the Eros plates was no exception to this rule. Some of these difficulties arose in the course of the measurement at the separate observatories and were vanquished as they arose. Particularly was this the case at our national observatory at Greenwich, where a complete determination of the distance of the planet was made from the Greenwich plates alone, without help from those of any other observatory, and with very satisfactory results. But to get the full advantage from all the many photographs taken it was necessary to co-ordinate all the measures made at the different observatories, which brought to light a new crop of difficulties. It is to the lasting credit of Mr. A. R. Hinks, of the Cambridge University Observatory, that he undertook, as a volunteer but with the full approval of the President of the Committee charged with the work, to collect and co-ordinate all the results. The labour was very heavy and has occupied a large part of his working time during ten years. The difficulties which cropped up were new at

every turn and great ingenuity was called for in overcoming them

One such difficulty may be mentioned in illustration It has been remarked above that, when stars are observed visually, there is apt to be a "magnitude equation," *i e* a difference between the records for bright and faint stars, but that the introduction of the photograph seemed to offer a check on these errors, being itself free from them On this assumption Mr Hinks proceeded to treat the measures of the Eros plates , but on comparing the measures at different observatories, he found between them just such differ ences as affect old visual observations under the head of magnitude equation The differences were not so large perhaps but were nevertheless sensible It may occur to the reader to inquire whether the differences arose in the measurements of the plates which were of course made visually , but such a possibility was eliminated by the method of measuring each plate twice over, as has been explained in a previous article, the plate being turned completely round for the second set of measures , any magnitude

equation would affect the two sets of measures in reverse directions and could thus be both detected and eliminated This alternative being ruled out, it followed that the error must be in the plates themselves, and it was a very disturbing discovery to find that we had not, as had been hoped, freed ourselves from such a kind of error by the introduction of photography

Some comfort was forthcoming from the further discovery that many of the plates *were* sensibly free from this error, but these only increased the puzzle What could be influencing those which showed unmistakable traces of it? Ultimately the cause was found in a faulty lens or rather in the faulty arrangement of the pair of lenses which go to make up the object glass of a telescope We have now realised that this arrangement must be carefully made, and that faults render us liable to this old trouble, but it also seems probable that with care on the part of the instrument maker, the trouble can be avoided or, at any rate, rendered conveniently small It is easy to sum up in a few words in this way the net result of

the investigation, but the investigation itself was a long and tedious one and is perhaps even yet scarcely complete It was faced, along with many others, with great courage and patience, with the ultimate result that, in the spring of 1909, Mr Hinks was able to announce to the Paris Academy of Sciences a most satisfactory result for the distance of the planet and, by implication, for that of the sun and of the other members of the solar system [1]

He was also able to add a value for the mass of the moon It may seem strange that this altogether different measurement of mass is to be deduced from the figures which give us a measure of length, but the fact is that we measure the mass of the moon by noting a certain length, namely, the distance by which it pulls the earth from side to side as it waltzes round with it The earth and the moon may be compared to a pair of partners dancing round a ballroom, if they were of equal size, they would swing equally to right and left of their average path, but the moon is much the

[1] See Note XI

smaller and only pulls the earth a very little way from side to side Nevertheless the oscillation is perceptible and it alters the aspect of the planet Eros in the same kind of way as the oscillation of a telescope from one side of the earth to the other Indeed the two oscillations are combined together in the measures and we only separate them by the fortunate circumstances that one takes place in a day and the other in a month

Before leaving the conclusion of this great problem of the planetary distances, which has come down to us through the ages, a word or two may be devoted to its history The Greeks made attempts to determine the sun's distance, but they were very crude for example Aristarchus of Samos made it only nineteen times the distance of the moon or about $4\frac{1}{2}$ million miles, and it was long before anything like the true value (about 93 millions) was arrived at In the middle of the nineteenth century the margin of doubt was some millions of miles, but it was expected that the transits of Venus in 1874 and 1882 would reduce this margin within narrow limits The observations

made at these famous transits were however very disappointing, and even before the second of them was due, some astronomers had already turned to other methods for finding the sun's distance, especially the observation of the planet Mars and later of one or other of the small planets. The best determination of this kind, previous to the Eros determination, was that by Sir David Gill at the Cape of Good Hope about 1889, who obtained a result closely like that at which Mr. Hinks arrived twenty years later. But there are other measurements which bear an interesting relation to this direct measure of distance, especially those which compare the velocity of the earth in its revolution round the sun with the velocity of light. This comparison can be made in two entirely independent ways which take account of two entirely independent movements of a star, one directly in the line of sight and the other at right angles to it. Until the invention of the spectroscope, the latter was the only movement of a star, real or apparent, of which we could take account. It was discovered by Bradley early in the

eighteenth century that by noting the apparent changes in direction of any star, we could find the ratio of the earth's velocity to that of light; for the aberration, as he called it, was due to the relation of these two velocities. Hence if we can find the velocity of light by independent means, we can deduce the velocity of the earth and from this the length of its path during one year; from this the radius of its orbit can be found, which is the quantity we seek. Now the velocity of light has been successfully measured by terrestrial experiments with sufficient accuracy, so that the distance of the sun can be deduced from measures of aberration. But curiously enough the value so found does not quite accord with that given by Sir David Gill and confirmed by Mr. Hinks. The discrepancy has been rendered more remarkable within the last year or two by the successful measurement of aberration by the other method, using the spectroscope, which enables us to measure the velocity of a star in the line of sight. The velocity thus found is partly that of the star and partly that of our earth; in

many cases (though not in all) we may consider that of the star as steady, but that of the earth varies during the year, being sometimes towards a particular star and sometimes directly away from it. By comparing the observations on these two occasions, we can eliminate the steady velocity of the star and deduce the velocity of the earth alone, from which we get, as before, the distance of the sun.

Now measures made recently on this plan have given a result in satisfactory accordance with that of Gill and Hinks, and have thus rendered the isolation of the other result from aberration the more remarkable. There are some who think that the discrepancy will ultimately lead us to the discovery of some new phenomenon about which we are at present entirely in the dark. To illustrate what is in their minds we may recall that Lord Rayleigh was led to the discovery of Argon by paying attention to minute discrepancies in the values he obtained for the density of nitrogen from different sources; and not only was the discovery of Argon important in itself but it has led to others

of vast importance So that all these may be said to have originated in the study of a minute discrepancy between two measures of what purported to be the same quantity Is it possible that the future may have in store for us similar weighty consequences, traceable to the study of this discrepancy in the measure of the sun's distance?

But astronomers know only too well how easily such discrepancies may turn out to be due to some source of error that has been overlooked Their science is concerned, perhaps more than any other science, with minute measurements which a minute error will nullify or disturb, and they must be continually ready to see the edifices which they have spent some labour in building tumble down like a house of cards owing to some tiny flaw in the foundations An instance of this occurred as a by product of the Oxford measures and will serve as an illustration In the year 1902 Sir David Gill made the suggestion that the brighter stars were apparently rotating as a whole with respect to the fainter stars as a whole, basing it upon many thousands of observa-

tions made at two epochs about half a century apart. If the whole universe was rotating together we might not be able to perceive it. Many familiar tests would fail just as they failed to reveal the rotation of the earth to our ancestors. Should we have yet learnt this great fact if our sky had been permanently cloudy so that we never saw the stars? We might have suspected it from the recurrence of daylight, and we might have actually inferred it if we could have surveyed the earth in some way and found its equatorial bulge, which we might have rightly ascribed to the effects of rotation. Similarly we might be able to infer the rotation of the whole universe of stars if we can be sure of its equatorial bulge of which the Milky Way is a possible manifestation. That brilliant thinker Henri Poincaré has made a rough estimate of a superior limit to such a rotation in his book *Science et Méthode* (p. 285), finding a second of arc in 3,000 years, or a complete rotation in four thousand million years, which is really not very long considering that geologists would like to take it all for the life of our earth itself.

But Sir David Gill was not dealing with this general rotation of all the stars together: he thought he had detected a *relative* rotation of the bright stars and it seemed possible that some evidence might be gathered from the photographic measures in the following way. Of the stars whose places had been determined at Cambridge about 1880, some had been photographed at Oxford in 1892, others (say) in 1902. Assuming that there was in reality a relative drift of the bright stars as suggested, the plates of 1892 ought to show ten years of it when compared with the Cambridge observations of 1882, whilst those of 1902 would show twenty years of it. By simple subtraction we could get ten years of the drift. The subtraction is rendered necessary by the existence of the "magnitude equation" already noticed at the beginning of this article, which would affect both determinations of drift and prevent the drift from being identified from either source by itself, though it could be found from their difference if it could be rightly assumed that the effect of magnitude equation was the same in both cases. The ex-

periment was accordingly tried, though scarcely under such favourable conditions as sketched above with the result that a drift of the bright stars seemed to emerge of about the magnitude assigned by Sir David Gill *but in the contrary direction* (*Mon Not R A S* lxiii p 56) Attempts were made to find a reason for the discrepancy but on extending the enquiry to movements in declination (*Mon Not* lxiv p 3), proper confirmation was not forthcoming, and it was suspected that there was some unknown source of error (*loc cit* p 18) At that time it had not been suspected that magnitude equation could occur in photographic measures, but when subsequently Mr Hinks came across a gross case of it in the work on the Eros photographs, as above mentioned, it was seen that the unknown source of error had probably been detected and the significance of the measures was thus destroyed (see *Mon Not* lxv p 55)

But if in this instance we failed to obtain what was searched for with much labour, on another occasion we made a considerable find without looking for it at all The his-

ory of our Oxford portion of the map was made remarkable by the quite unexpected discovery of a New Star. It would be possible to institute a regular search for new objects by the use of star maps, comparing one plate with another taken on a different date; again, if the spectra of the stars are photographed as at the Harvard Observatory, then a new star might reveal itself on inspection of a single plate by the peculiarity of its spectrum. But neither of these methods was in the least degree in our minds in the course of the Oxford work on the map and the discovery was entirely accidental, as will be seen from the following account.

At the beginning of the year 1903 we were within sight of the completion of the measures and hoped to reach it before the end of the year. For several reasons the actual completion was ultimately delayed beyond this date but that is a point which does not concern us just now. In the hope of completing the measures before the end of 1903, we were making great efforts to secure all the plates which had not yet been taken

If the favourable season for taking a particular plate before it "runs into daylight" is lost, we may have to wait nearly a year before another opportunity recurs; so that it was important to obtain all the January plates in January 1903, not leaving any gaps for January 1904. To expedite matters, when there came a specially fine night or two, a large number of plates were taken, which were set aside for development until the good weather was gone. In England we have learnt to prize these exceptional nights; and it may be remarked in passing that we occasionally get nights as good as anywhere in the world, though the occasions are not so frequent as in California, for instance. All too soon the indifferent weather came, the plates were developed and, to our great disappointment, it was found that they were not satisfactory. There had been an unfortunate failure in sensitiveness of the films, which is apparently liable to happen in the manufacture of extremely rapid plates; when straining at the limit of sensitiveness some very slight cause may produce a notable failure to reach that limit

The disappointment was the greater because it was practically the first of the kind, throughout the whole work the plates had been uniformly satisfactory, in spite of the risks just mentioned, otherwise we might perhaps have been on our guard against the short coming There was, however, nothing for it but to take the photographs again, and if there had been need for special exertion before, this need was now much greater in consequence of the diminished time at disposal No very great surprise therefore was felt when one or two of the new plates were found to be faulty from a different reason There was no further failure in sensitiveness, for the plate makers were most sympathetic about our disappointment and immediately furnished an excellent batch of new plates The fault was now that the telescope had not been accurately pointed to the right region of the sky—a kind of mistake which might reasonably be ascribed to the strain of working against time But it is a good rule in astronomical work (probably also in other walks of life) to get to the bottom of any mistake if possible, an

so it proved in this instance On comparing one of the wrongly set plates with another of the same region, it was seen that it contained a strange object which ultimately proved to be a New Star The mistake had arisen because it is customary to select as guiding star the brightest in the neighbourhood (as being most easily identified) and the new star had blazed up so as to be brighter than any other near it, so that Mr Bellamy had accepted it without question as the one to which he was to point his guiding telescope during the taking of the photograph

We could not be sure for some little time of the nature of this object It might be a planet or a variable star The first alternative was soon disposed of, because it is easy to look up the places of the planets which could be bright enough, and, moreover, a planet would probably have betrayed itself by a slight movement between the three exposures given to each plate in the making of the map The second alternative occupied attention rather longer There are many stars scattered over the sky whose

brightness varies considerably, so that they might at one time show an emphatic image on one plate and at another time be too faint to affect the plate at all Many of these are well known and can be found in catalogues already published, others are being discovered year by year and no doubt we are still unaware of many to be discovered in the future During the afternoon th lists were searched without finding any mention of a "variable" in that particula neighbourhood, and when in the evenin the star was found to be still shining in th exact place of the photograph, telegram were sent to other observers inviting the attention to it as probably a New Sta Any remaining doubts were dispelled by th spectroscopic observations, and Nova Gem norum took its place as No 18 in the li of Novæ which had been discovered in th history of astronomy

Readers of the daily press will probab have seen recently an announcement of similar discovery by the Rev T E Espi of Darlington, which is No 22 in this lis for during the past autumn no less th

three special objects were discovered at the Harvard Observatory from the examination of photographic plates. The total number is, however, still not large, though from the facts that up to the year 1884 only eight had been recorded and that the other sixteen have all been found in the last quarter of a century, we may infer that the rarity is partly due to our own lack of vigilance, and that the few discoveries recorded would probably have been supplemented by many others had a more systematic watch been kept.

We are at present not very well informed as to the nature of the celestial event which is represented by the appearance of a new star. A few things about it we know. In the first place the event is a sudden one, the light of the star increasing enormously within a day or two by something like twelve magnitudes—that is to say, in a ratio of about 1 to 80,000—then the light slowly diminishes—slowly but not quite steadily; there are fluctuations in the course of the diminution, and these fluctuations were specially noticeable in the case of the new

star of 1901—in Perseus. Sir Robert Ball gave us, at the Royal Astronomical Society, an amusing account of his experiences at the time when the fluctuations were such as would cause the star first to disappear to the naked eye and then to reappear again. He had taken a party of visitors into the open to show them the new star, only to find that it had disappeared; on the next night he took out another party to show them the disappearance and, as though to spite him, it had reappeared again. But these were only temporary vagaries, as the star was soon permanently lost to our sight and then even to telescopes of moderate power. It still remains, however, as a very faint object visible in large telescopes.

Another incident in the history of this particular new star may be noticed, for it seems to tell us something about the origin of such objects. When the light had become very faint, so that photographs of the region were necessarily taken with long exposures, there was found to be a faint nebulous light surrounding the star, and successive photographs showed that this

nebulous appearance was expanding in all directions, just as though there had been an explosion and the fragments were still flying outwards The phenomenon aroused the greatest possible interest, for a rapid change —that is to say, any change which is perceptible in a few weeks—is almost unprecedented in the case of the stars and could have only one of two explanations either the star is specially close to us so that the changes *appear* larger than usual or, if the star be at a distance similar to those of other stars, the changes themselves must be on a gigantic scale It was soon seen that the latter was the right alternative and it was inferred that the velocities of the flying fragments must be comparable with the velocity of light (nearly 200,000 miles a second)

Now there is an interesting physical question, whether it is possible for gross matter to move through the ether with a velocity greater than or even as great as that of light At first the hope was entertained that we were going to get some information on this interesting question, but a more practical

alternative was suggested, viz that the velocity exhibited was not that of matter but actually that of light itself The observed facts would be explained if the nebula had been in existence previously but had been without illumination, so that we were unaware of it, just as we are unaware of an object in a dark room until a flash o lightning illuminates the room In such case the illumination appears to be instan taneous, but since light does actually tak time to travel, it cannot be quite instan taneous, which we should realise were tl room billions of miles in size The roon taken up by a nebula is of this size an the flare-up of the new star therefore illu minated it gradually, beginning with tl nearer portions and spreading to those mo distant as time went on This explanatio of the facts was confirmed by a remarkab experiment The light of the nebula w analysed by means of the spectroscope an found to correspond with that of the origin flare A spectrum is, after all, only a glo fied name for a colour, we may represe the facts in simple language by substituti

names of colours. The events would then be as follows: the star rose to its greatest brightness with a blue light, which afterwards turned to red and remained red as the light died away. Now the light of the nebula was not found to be red, as it would have been if it belonged to the star in its later stages, but was found to be blue, and must therefore owe its existence to the blue flare some months previous, which had taken that time to traverse the huge distances separating the outlying portions from the centre. If this be so, we may further suspect the nebula of having been concerned in some way in the original outburst. It seems plausible that some kind of encounter between the previously faint star and the previously faint nebula should have resulted in a great development of heat and light which sent the news to us.

It would be interesting to get confirmation of this possibility in other cases, but unfortunately the conditions are not always so favourable. Nova Persei blazed up brighter than the first magnitude stars, and though there have been New Stars even

brighter than these (such as that of 1572 which was even visible in the daytime), most of those we now find are much less bright, so that if they are accompanied by the illumination of nebulæ our resources are not able to photograph them

Here we must conclude this brief review of a quarter of a century's work on the Great Star Map and other matters related to it The work is far from concluded as a whole, though two portions of it have been so far finished as to enable us to form some idea of the completed whole But the attainment of any particular stage is after all only an incident in a journey, for in a very real sense the map will never be finished Our real concern is not with the state of the heavens at any particular moment, but with the changes which may be discerned by comparing one epoch with another, accordingly when we have mapped out any region satisfactorily we are not at the end but at the beginning Our real work consists in watching the development of change, which may be slow to declare itself to our brief lives but will persist relentlessly during eternit

# NOTES

## I

## ASTRONOMERS PRESENT AT THE CONFERENCE WHICH MET AT PARIS ON APRIL 16, 1887 AND FOLLOWING DAYS

U S AMERICA (3)
- Elkin
- Peters
- Winterhalter

ARGENTINE REP (1)
- Beuf

AUSTRALIA (1)
- Russell

AUSTRIA (2)
- Eder
- Weiss

BELGIUM (1)
- Folie

BRAZIL (1)
- Cruls

CAPE OF GOOD HOPE (1)
- Gill

DENMARK (2)
- Pechüle
- Thiele

ENGLAND (6)
- Christie
- Common
- Knobel
- Perry
- Roberts
- Tennant

FINLAND (1)
- Donner

FRANCE (20)
- Baillaud
- Bertrand
- Bouquet de la Grye
- Cloué
- Cornu
- Faye
- Fizeau
- Gautier
- Henry (Paul)
- Henry (Prosper)
- Janssen
- Laussedat
- Liard
- Loewy
- Mouchez
- Perrier
- Rayet
- Tisserand
- Trépied
- Wolf

GERMANY (6)
- Auwers
- Krueger
- Lohse
- Schoenfeld
- Steinheil
- Vogel

HOLLAND (3)
Bakhuyzen
Kapteyn
Oudemans

ITALY (1)
Tacchini

PORTUGAL (1)
Oom

RUSSIA (2)
Hasselberg
Struve

SPAIN (1)
Pujazon

SWEDEN (2)
Dunér
Gyldén

SWITZERLAND (1)
Gautier

## II

## SOME OF THE LEADING RESOLUTIONS OF THIS FIRST CONFERENCE

1 Les progrès réalisés dans la Photographie Astronomique exigent impérieusement que les astronomes de notre epoque entreprennent en commun la description du Ciel par le moyen des procédés photographiques

2 Ce travail sera fait dans des stations à choisir ultérieurement, et avec des instruments qui devront etre identiques dans leur parties essentielles

3 Les buts principaux seront

(*a*) De dresser une Carte photographique générale du Ciel pour l'époque actuelle, et d'obtenir des donnees qui permettront de fixer des positions et les grandeurs de toutes les étoiles, jusqu'a un ordre determiné, avec la plus grande précision possible (les grandeurs étant entendues dans un sens photographique à définir)

(*b*) De pourvoir aux meilleurs moyens d'utiliser,

tant à l'époque actuelle que dans l'avenir, les données fournies par les procédés photographiques

4 Les instruments employés seront exclusivement des réfracteurs

(The remaining 20 resolutions are more technical )

## III

## LIST OF OBSERVATORIES WHICH ORIGINALLY UNDERTOOK THE WORK OF THE GREAT STAR MAP

| Observatory | Nation | Position on Earth | | Belt of Sky | No of Plates |
|---|---|---|---|---|---|
| | | Lat | Long E h m s | | |
| Greenwich | British | +51 29 | 0 0 0 | +90 to +65 | 1149 |
| Rome(Vatican) | Italian | +41 54 | 0 49 49 | +64 to +55° | 1040 |
| Catania | Italian | +37 30 | 1 0 20 | +54° to +47° | 1008 |
| Helsingfors | Russian | +60 10 | 1 39 49 | +46 to +40 | 1008 |
| Potsdam | German | +52° 23 | 0 52 16 | +39° to +32 | 1232 |
| Oxford (Univ ) | British | +51 46 | 23 55 0 | +31° to +25 | 1180 |
| Paris | French | +48° 50 | 0 9 21 | +24 to +18 | 1260 |
| Bordeaux | French | +44 50 | 23 57 54 | +17 to +11° | 1260 |
| Toulouse | French | +43 37 | 0 5 50 | +10 to + 5° | 1080 |
| Algiers | French | +36 48 | 0 12 8 | + 4 to − 2° | 1260 |
| San Fernando | Spanish | +36 28 | 23 35 11 | − 3 to − 9° | 1260 |
| Tacubaya | Mexican | +19 24 | 17 23 13 | −10 to −16 | 126[illegible] |
| Santiago | Chilian | −33 27 | 19 17 14 | −17 to −23 | 126[illegible] |
| La Plata | Argentine | −34 55 | 20 8 23 | −24° to −31 | 136[illegible] |
| Rio de Janeiro | Brazilian | −22 54 | 21 7 19 | −32 to −40 | 137[illegible] |
| Cape of Good Hope | British | −33° 56 | 1 13 55 | −41 to −51° | 151[illegible] |
| Sydney | Australian | −33 52 | 10 4 50 | −52° to −64 | 140[illegible] |
| Melbourne | Australian | −37° 50 | 9 39 54 | −65° to −90 | 114[illegible] |

In 1900 the three zones assigned to Santiago, La Plata, and Rio de Janeiro, which had not been commenced, were assigned to Monte Video, Cor

doba (Argentine), and Perth (W. Australia) respectively. The last two are at work on their zones, but Monte Video was not able to make a start, and the zone was in 1909 reassigned to Santiago and Hyderabad conjointly.

## IV

## TABLE SHOWING THE POSITIONS OF THE CENTRES OF THE PLATES IN THE SKY

THE length of a belt of the sky parallel to the equator decreases as we approach the poles, slowly at first, but rapidly later. The area of sky covered by a single plate remains the same and hence we require fewer plates to go round the belt, unless we allow them to overlap. A small overlap is arranged for in any case, as an obvious precaution and by the arrangement of plates adopted this is allowed to grow until a convenient change can be made. Thus at the equator it takes 180 plates, each 2° wide, to go round the whole circuit of 360°. A belt parallel to the equator, but 20° distant from it is smaller and would only require 180 × ·939 or 169 such plates, without overlap. We could therefore save 11 plates if we liked. But this economy is not considered sufficient to outweigh the convenience of having the centres of the plates placed exactly one above the other in the sky, instead of being at unequal intervals. Accordingly the plate centres are chosen to be exactly one

over the other for a belt 26° wide on each side of the equator It must be remembered however that there are two series of plates, covering the sky twice over and the centres of the second set are placed at the corners of the first

Hence if the centres of the first set be at

| | | | | | |
|---|---|---|---|---|---|
| | | h m | h m | h m | h m |
| Decl 0°, | RA | 0 0 | 0 8 | 0 16 | 0 24 &c |
| Decl 2 | RA | 0 0 | 0 8 | 0 16, | 0 24 &c |
| Decl 4°, | RA | 0 0, | 0 8, | 0 16, | 0 24 &c |

then those of the second set will be exactly intermediate both in declination and RA *i e* they will be at

| | | | | |
|---|---|---|---|---|
| | | h m | h m | h m |
| Decl 1° | RA | 0 4, | 0 12 | 0 20 &c |
| Decl 3 | RA | 0 4, | 0 12, | 0 20 &c |

On reaching 28° of declination the interval between the plates is changed to $9^m$ of RA instead of $8^m$, so that 160 of them suffice to make the circuit Their centres are at

| | | | |
|---|---|---|---|
| | h m | h m | h m |
| RA | 0 0 | 0 9 | 0 18 &c |

and so are those of zones 30°, 32°, 34°, and 36° The centres of the second set should thus, to fulfil the arrangement exactly, be at

| | |
|---|---|
| h m | h m |
| 0 4½, | 0 13½ &c |

but it was felt that the half minute might introduce inconveniences and it was dropped, so that the centres are placed at

| | |
|---|---|
| h m | h m |
| 0 4 | 0 13 &c |

With these few words of explanation, the general reasons for the following arrangement will be tolerably clear

## POSITIONS OF PLATE CENTRES FIRST SET

| Declinations | | Right Ascensions | No in Zone |
|---|---|---|---|
| | | h m h m h m | |
| 0 2 4 | to 26 | 0 0 0 8 0 16 &c | 180 |
| 28 30 | to 36 | 0 0 0 9 0 18 &c | 160 |
| 38 40 | to 48° | 0 0 0 10 0 20 &c | 144 |
| 50 52 | to 58 | 0 0 0 12 0 24 &c | 120 |
| 60 & 62 | | 0 0 0 16 0 32 &c | 90 |
| 64 & 66 | | 0 0 0 18 0 36 &c | 80 |
| 68 & 70 | | 0 0 0 20 0 40 &c | 72 |
| 72° & 74 | | 0 0 0 24 0 48 &c | 60 |
| 76 & 78 | | 0 0 0 30 1 0 &c | 48 |
| 80 & 82 | | 0 0 0 40 1 20 &c | 36 |
| 84 | | 0 0 1 0 2 0 &c | 24 |
| 86 | | 0 0 1 30 3 0 &c | 16 |
| 88 | | 0 0 2 0 4 0 &c | 12 |

and a single plate at the Pole

## POSITIONS OF THE SECOND SET

| Declinations | | Right Ascensions | No in Zone |
|---|---|---|---|
| | | h m h m h m | |
| 1 3 5 | to 27 | 0 4 0 12 0 20 &c | 180 |
| 29 31 | to 35 | 0 4 0 13 0 22 &c | 160 |
| 37 39 | to 47 | 0 5 0 15 0 25 &c | 144 |
| 49 51 | to 59 | 0 6 0 18 0 30 &c | 120 |
| 61 & 63 | | 0 8 0 24 0 32 &c | 90 |
| 65 | | 0 9 0 27 0 45 &c | 80 |
| 67 & 69 | | 0 10 0 30 0 50 &c | 72 |
| 71 & 73 | | 0 12 0 36 1 0 &c | 60 |
| 75 & 77 | | 0 15 0 45, 1 15 | 48 |
| 79 & 81 | | 0 20, 1 0 1 40 | 36 |
| 83 & 85 | | 0 30 1 30 2 30 | 24 |
| 87 | | 0 45 2 15, 3 45 | 16 |
| 89° | | 1 30, 4 30, 7 30 | 8 |

## V

## NUMBER OF STARS IN THE OXFORD ZONES

As will be seen from the list of observatories (III above) the zones with plate centres in declinations +25° to +31° were assigned to the University Observatory, Oxford The measures were completed in 1904, and the following figures will give some idea of the work The total number of plates measured was 1180, so that in each of the 24 hours of RA there were (a fraction over) 49 plates, though the arrangement of centres makes the figures vary a little sometimes 50 or 51 and sometimes 48 being credited to a particular hour For the sake of uniformity a correction has been applied to reduce each hour to a fictitious 49 plates, and the number of stars is given (to the nearest 50, to avoid useless refinement) in the second column of the table

In the third column is given the average ratio of the stars measured to those shewn in Argelander's charts, which represent the most extensive previous information of the kind It will be seen that the number has been increased more than 4 times, while the increase in *accuracy* (not, of course, shewn in the table) is perhaps 20 times The Milky Way crosses the Oxford zone at $5^h$—$6^h$

and again at $19^h$—$20^h$, and it will be seen how greatly the numbers increase in these regions

| h | Total Stars on 49 Plates | Ratio to Argelander |
|---|---|---|
| 0 | 12800 | 4.0 |
| 1 | 18400 | 4.6 |
| 2 | 12550 | 4.7 |
| 3 | 12650 | 4.7 |
| 4 | 8450 | 3.3 |
| 5 | 22050 | 4.8 |
| 6 | 24850 | 4.0 |
| 7 | 17650 | 4.2 |
| 8 | 18650 | 4.1 |
| 9 | 10300 | 4.0 |
| 10 | 9450 | 4.1 |
| 11 | 7600 | 3.9 |
| 12 | 8450 | 4.0 |
| 13 | 7900 | 4.0 |
| 14 | 8850 | 4.0 |
| 15 | 9600 | 4.1 |
| 16 | 12750 | 4.7 |
| 17 | 22800 | 6.2 |
| 18 | 26500 | 5.5 |
| 19 | 48600 | 5.9 |
| 20 | 88800 | 6.0 |
| 21 | 23100 | 4.9 |
| 22 | 18500 | 4.6 |
| 23 | 18850 | 4.1 |
| Total | 398600 | |

Some of the plates taken in the early years showed an insufficient number of stars These have now been taken again, and the total number of stars has been thereby increased by about 73,000

## VI

## SIZE AND WEIGHT OF PUBLICATIONS

THE Map consists of two different parts (*a*) *The Charts*, which are enlargements (by heliogravure or other photographic process) of the *long* exposure plates on twice the original scale (linear), (*b*) *The Catalogue*, giving the measures of the *short* exposure plates

(*a*) It seems improbable that more than a portion of the charts will ever be published The publication is very costly (from £5,000 to £10,000 per observatory) and can only be afforded by the wealthier observatories But the part of the sky containing the ecliptic will probably be completed, as it has been definitely undertaken by the French nation The charting of the ecliptic was indeed undertaken by them before the advent of photography, and they have shown a most praiseworthy desire to carry it to accomplishment by the new method The Paris charts are on paper 17 in × 22 in and the 1260 charts of this observatory will make a pile of paper of this area 22 inches high weighing about 280 lbs If the whole sky were completed in the same way the pile of paper would be 32 feet high and weigh some 4,000 lbs Various devices have been adopted for storing the charts but the most successful is the "vertical filing system recently introduced at the Royal Astronomical Society

(*b*) The Catalogue is being published in very different ways by the different observatories Potsdam for instance is publishing the measures as they are made, with no reference to the order of plates in the sky The printing is on a generous scale, 57 plates occupying a volume 12 in $\times$ 10 in $\times$ $1\frac{1}{2}$ in, weighing 5 lbs On this scale there will be 22 volumes for the Potsdam plates alone, occupying $2\frac{1}{2}$ feet of shelf room and weighing 110 lbs On the same scale the whole catalogue would consist of 887 volumes, occupying 45 feet of shelf room and weighing nearly a ton But other observatories have adopted a more compact arrangement The Greenwich measures are completely published in two volumes weighing 17 lbs altogether and occupying only $5\frac{1}{2}$ inches of shelf room—less than one-fifth of the Potsdam dimensions and the Oxford portion in 7 volumes occupying $7\frac{1}{2}$ inches when bound Nevertheless even on the Greenwich scale the weight of the catalogue would be considerable—say 330 lbs, without the extra volumes devoted to discussions of results

## VII

## ACCURACY OF THE RESEAUX

THE reseau is a piece of plate glass coated with silver and ruled with two sets of cross lines $5^{mm}$ apart, and at right angles to each other If placed nearly in contact with a photographic film and exposed to parallel light, the light only pene-

trates through the ruled lines, which are thus photographed and appear together with the star images when the plate is developed In the measurement of the plate these lines are treated as exactly equally spaced and exactly at right angles, but of course

($a$) The lines may not be equally spaced, *e g* the average interval in the $x$ direction may be $5^{mm}$ exactly, and in the $y$ direction may be a little more or a little less In this case the "scale value" of the $x$ measures will differ from that of the $y$ measures If we photograph such a reseau twice on the same plate, turning it round through 90° between the exposures (so that $x$ lines of one exposure fall near $y$ lines of the other) we can readily check this error by measuring the $x$ spaces between the lines for different values of $x$, and the $y$ spaces for different values of $y$

($b$) The lines may not be at right angles This would give a different "orientation" in $x$ measures and $y$ measures The error can be checked by the same double exposure, from the variation of $x$ spaces with $y$ (or $y$ spaces with $x$)

Instances are given of these general errors, and of their detection from measures on stars before they were measured as above, in *Mon Not R A S*, lxxi pp 117–8 The difference in spacing is about 1 in 10,000 and the deviation from a right angle is about 30′, which is a quantity of the same order

($c$) In addition to general or average defects such as these, the individual lines may be wrongly

spaced or may be inclined to the others, or may be curved instead of straight No line is perfect, but the errors are generally small enough to be neglected *for the work of the Astrographic Catalogue*, which is a moderately rapid survey of a very large number of stars For refined investigations however these errors must undoubtedly be taken into account

In such cases it does not however suffice to determine the errors of the reseau in the laboratory, for they are not exactly reproduced in the photo graphic copy Every copy varies slightly from every other Indeed when we attempt to attain a greater accuracy than has been aimed at in the work on the Map, we find a considerable number of new difficulties

Nevertheless it may safely be said that the reseau is of immense value in all photographic work A careful determination of errors of the reseau at 378 different points is given in the Introduction to Vol I of the Greenwich portion of the Catalogue The errors are all less than 0 001 of a reseau interval, and in

| | | | | | | | | | |
|---|---|---|---|---|---|---|---|---|---|
| 219 | cases | are | less than | | 0001 | | | | |
| 118 | ,, | ,, | greater | than | 0001 | but | less | than | 0002 |
| 24 | ,, | ,, | , | ,, | 0002 | ,, | ,, | ,, | 0003 |
| 14 | ,, | ,, | ,, | ,, | 0003 | ,, | ,, | ,, | 0004 |
| 6 | ,, | ,, | ,, | ,, | 0004 | ,, | , | , | 0005 |
| 2 | ,, | ,, | , | ,, | 0005 | , | , | ,, | 0010 |

The two exceptional cases are errors of 00055 and 00082, at single points only

# VIII

## SPECIMEN OF A PORTION OF THE OXFORD CATALOGUE

The main features of the Oxford Catalogue are shown by this extract from the first volume, which gives measures of stars on plates with centres in declination +31° (shown at top of page) To each plate there is a heading showing first the R A of the plate centre, then the number of the plate in the Oxford series, and the date on which it was taken, and then six constants A, B, C, D, E, F, which enable us to correct the measures to form "standard co ordinates," *i e* imaginary measures on an ideal plate, correctly fitted to the sky

+31°

R A 15h 40

Plate 2250 1903 Apr 22

*Provisional Constants*

| A | B | C |
|---|---|---|
| − 00082 | − 00211 | − 0115 |
| **D** | **L** | **Γ** |
| + 00238 | − 00055 | + 0037 |

$Mag = 17\ 2 - 1\ 30\sqrt{d}$

| No | $d$ | $x$ | $y$ |
|---|---|---|---|
| 34401* | 36 | 0 395 | 0 437 |
| 34402 | 30 | 4 585 | 0 030 |
| 34403* | 38 | 8 280 | 0 386 |
| 34404 | 17 | 9 328 | 0 114 |
| 34405 | 9 | 10 073 | 0 733 |
| 34406 | 20 | 11 840 | 0 355 |
| 34407 | 10 | 12 500 | 0 564 |
| 34408 | 23 | 15 590 | 0 017 |
| 34409 | 14 | 15 890 | 0 027 |
| 34410 | 15 | 16 034 | 0 809 |
| 34411 | 13 | 19 260 | 0 099 |
| 34412 | 10 | 19 782 | 0 973 |
| 34413 | 12 | 21 295 | 0 029 |
| 34414 | 14 | 21 464 | 0 687 |
| 34415 | 14 | 23 477 | 0 426 |
| 34416 | 19 | 4 220 | 1 912 |
| 34417 | 12 | 4 313 | 1 956 |
| 34418 | 13 | 4 939 | 1 047 |
| 34419 | 22 | 6 147 | 1 700 |
| 34420* | 40 | 11 331 | 1 220 |
| 34421 | 8 | 15 344 | 1 450 |
| 34422 | 9 | 16 600 | 1 147 |
| 34423 | 14 | 25 506 | 1 609 |

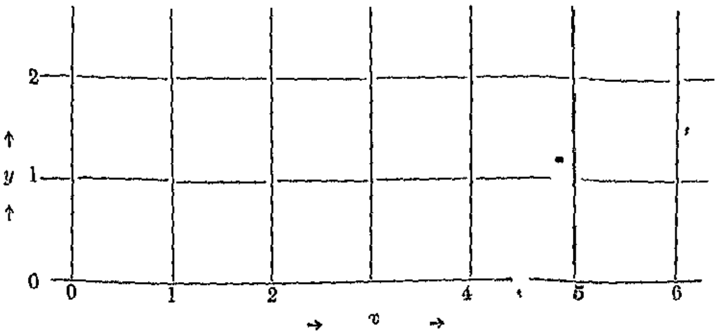
The formula "Mag" enables us to find the approximate brightness of the star from the mea

sured diameter of its photographic image (d), which is given in the second column below. The first column is merely a running number for reference, and the asterisks mean that the star marked is one which has been observed with the transit circle at another observatory, giving information which enables us to find the above constants A, B, C, etc. As regards the second column, it need only concern us to remark that a bright star has a large image and a faint star a small one. Thus the first star has d=36 and is bright. To find how bright we take the square root of $d$ which is 6, multiply by 1.36 getting 8.16, and subtract this from 17.2 which gives 9.04 so that the star is of magnitude 9.04 as given by these measures. Star No. 34405 which has d=9 would be of magnitude 13.12 according to the formula. But this is probably too faint an estimate; the formula is not quite correct for faint images.

The columns for $x$ and $y$ give the position of the star, counting by the reseau squares and fractions of them; $x$ from left to right, and $y$ from bottom to top. There are 26 such squares each way, and each of them corresponds to 5′ on the sky; so that the whole width or height is 2° 10′. The following diagram of an enlarged portion of the plate will show the meaning of $x$ and $y$ more clearly.

# IX

## DIAGRAM OF AN ENLARGED PORTION OF A PLATE




The above diagram represents an enlargemen of a small portion of the plate of which a fe measures are given in the preceding note Th first star, 84401, is conspicuous in the lower lef hand corner then as we proceed along this botto line of squares to the right, the next three a empty, the second star (84402) having $x$=4 5 which puts it in the fourth square more than ha way across and $y$=0 080 which puts it close the bottom line There are three images of t star, produced by exposing the plate three tim to the sky in slightly different positions of t telescope It is the uppermost image of whi the position is recorded Hence a small star whi

has its lower images at the top of the square $x=4+$ is catalogued under $y=1+$, and not under $y=0+$ The rest of the stars catalogued in the preceding note with $y=0+$, *viz* Nos 34103 to 34415, cannot be shown on the small part of the plate which has been enlarged No 34416, the first in line $y=1+$, occurs at $a=4$ 220, and No 34417 is close to it The images have been unduly enlarged to render them visible at all on the diagram The whole plate consists of $26 \times 26$ squares $= 676$ in all, of which only 14 are shown above There are 308 stars on the whole plate, less than $\frac{1}{2}$ a star per square so that 6 stars in 14 squares is about the average

# X

## STATE OF THE WORK IN APRIL 1909

THE following summary is taken from the *Monthly Notices of the Royal Astronomical Society*, vol lxx p 876

| Observatory | No Plates in share | Catalogue Plates Measured | Catalogue Plates Reduced | Catalogue Plates Published | Charts Published |
|---|---|---|---|---|---|
| Greenwich | 1149 | 1149 | 1149 | 1149 | 1149 |
| Rome | 1040 | — | — | 51 | 32 |
| Catania | 1008 | 90 | — | 30 | — |
| Helsingfors | 1008 | 679 | 679 | 252 | — |
| Potsdam | 1232 | 300 | 280 | 207 | — |
| Uccle | | — | — | — | 32 |
| Oxford | 1180 | 1180 | 1180 | 820 | — |
| Paris | 1260 | ? | 540 | 360 | 359 |
| Bordeaux | 1260 | 819 | 540 | 360 | 119 |
| Toulouse | 1080 | 698 | ? | 186 | 177 |
| Algiers | 1260 | 517 | 425 | 280 | 333 |
| San Fernando | 1260 | 1125 | 323 | 0 | 225 |
| Tacubaya | 1260 | 1121 | 360 | 0 | 108 |
| Santiago / Hyderabad | 1260 | 0 | 0 | 0 | — |
| Cordoba | 1360 | 299 | ? | 0 | — |
| Perth | 1376 | 188 | 188 | 0 | — |
| Cape of Good Hope | 1512 | 1402 | 803 | 0 | — |
| Sydney | 1400 | 705 | ? | 0 | — |
| Melbourne | 1149 | 1149 | ? | 0 | — |

## XI

## THE RESULTS OF THE EROS CAMPAIGN

In the winter 1900–1 the recently discovered planet Eros was photographed by many of the co operating observatories for the purpose of determining the sun's distance The Greenwich results were dis cussed separately at Greenwich and the Lick results at that Observatory, but the whole series was collated by Mr A R Hinks of Cambridge He found the following value for the solar parallax from these photographic measures (*M N R A S* lxix p 567)

$$= 8\ 8067 \pm 0\ 0025$$

From visual observations made with micrometers he found (*M N* lxx p 603)—

$$\pi = 8\ 806 \pm 0\ 004$$

Incidentally he determined the mass of the moon to be the $1/n$th part of that of the earth where

$$n = 81\ 53 \pm 0\ 17$$

and the constant of Nutation to be

$$9\ 213$$

and the mechanical ellipticity of the earth to be

$$003278 = 1/305$$

## XII

On reading over the proofs Mr Eddington made the following important comment on the argument of pages 37–38

"One point may be rather misleading The disturbance of the ratio for the first $n$ magnitudes arises from the fact that you start from the sphere of radius 10 and ignore the infinite series of successively smaller spheres within it There will be glowing stars on the sphere of radius 6 3, which will be of the second magnitude, and when these are added in the ratio of second to third magnitude will again be as 1 to 4"

My reply, which has been accepted by Mr Eddington, is as follows —

"When an argument is stated in the language of everyday life, without the help of precise mathematical notation, it is often necessary to throw overboard some non essential details, though great care must of course be taken to retain anything essential

"The throwing overboard of the infinite series of inner spheres may be justified in the following manner

"Suppose that the stars, instead of being arranged at random, had been arranged in a regular pattern, so that the distance between any star and its nearest neighbours was *exactly* 10 throughout [We can imitate such a pattern by piling up billiard balls in a regular manner

every ball would have several neighbours touching it, and the distance between the centre of a ball and the centre of any ball touching it would be the diameter of a ball.]

"Then when we describe a sphere of radius 10 round any star as centre, its surface will contain all the nearest stars, and there will be none at all within it.

"To such a regular arrangement the argument of pages 87–88 will apply strictly, since we have an initial surface to start from.

"The infinite series of inner spheres to which Mr. Eddington rightly calls attention depends for its existence on the fact that the arrangement of the stars is not regular but irregular; and the difference between these two arrangements is therefore thrown overboard as a non-essential detail in the argument as stated.

"The argument must therefore not be applied as it stands to the existing system, but there is an exact proposition corresponding to it, *viz.* diversity of intrinsic brightness in the stars cannot explain a *decrease* in the ratio in question *near the Sun*, though it might explain an *increase*.

# INDEX